

INQUIRY

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Erratum: Inquiry, Vol. 1, No. 3, p. 163 line 9 read:

"The difference between the ordinary use and the philosophic use hinges rather upon the alleged fact that the ordinary man applies the word "voluntary" almost exclusively to actions which..."

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An Editorial Statement

This Journal seeks to fill a gap in current periodical literature. It will endeavour to intensify the debate between philosophers and social scientists and to stimulate research on problems common to philosophy and the various disciplines in the behavioural and social sciences. The Journal is intended to serve several groups and to accentuate the interests and the preoccupations which they have in common; it is intended for philosophers who are interested in problems concerning the foundations of the sciences of man and society; it is intended for behavioural and social scientists who are concerned with problems of theory-building, with the logic of analysis and inquiry, and with the ethical assumptions and the practical implications of their research; and it is, in particular, intended for all those who are trying to cut across the boundaries of various disciplines and to tackle analytically and empirically problems which cannot be solved within any single tradition of scholarship or research. We hope to be able to make this Journal a common meeting-ground for philosophers, psychologists, anthropologists, sociologists, economists, political scientists and jurists who are concerned with the basic problems of their sciences and who are aware of the need for continued clarification of the possibilities of effective integration which will cross the traditional borderlines between disciplines. This Journal will not in any way compete

with any of the periodicals which are established for the various disciplines; it seeks to supplement them by stimulating discussion and philosophical clarification of common problems.

The appeal of scientific research for investigators in these uncharted fields does not lie only in its instrumental value, for the direct results may indeed be modest when compared with the immense fields towards which their questions are directed; nor is the primary appeal of research activity a quest for certainty and order. Rather it is, first of all, the research attitude with its open-mindedness, uncertainty and wondering, which contributes to fruitful, expansive research in their chosen fields. In pilot-studies, pioneering and preliminary explorations, this attitude is constantly rewarded. 'Research' and 'working hypothesis' rather than 'science' and 'established result' will be among the central slogans of the Journal. Also catchwords such as 'the open society', 'the open self' appeal to us, stressing, as they do, the connection of the ethos of 'working hypothesis' with the basic ideals of society as well as of personality, thus inviting inquiry into one's self, one's presuppositions, one's culture and one's ability thereby to change and rise to more tolerant human perspectives. Such ideals are also closely related to the ideals of objectivity which are often essential in social and behavioural research: the ability consciously and honestly to take into account special personal presuppositions or tendencies, circumstances, weaknesses or limitations of methods etc., and to correct or allow for them in order to achieve more generally valid results.

Research activity is an expansive activity. All sciences are occupied with bridge-building and have their frontiers, their mixed disciplines with uncharted or perplexing meta- and foundation-problems. This is apparent when their development has not been hampered by rigid conceptual systems, conservative traditions and institutions, or political forces hostile to unrestricted freedom of research.

The fields to which the Journal is devoted are characterized by their bridge-building and frontier activity. It is therefore natural that contributions connecting apparently very different fields will be welcome; e. g., research connecting formal logic and developmental psychology as a part of a broader discipline, genetic epistemology, or research connecting jurisprudence and social psychology, general ethics and social problems.

The imminent *risk* of making unwarranted applications of principles and terminologies and of mixing together what does not belong to-

gether is a valuable stimulus at work on these frontiers. The general value of research is dependent upon the maintenance of a research attitude in favour of taking risks and admitting defeat when difficulties have not been overcome. Many research workers in the social sciences and in other fields undertake work stimulated by beliefs which are not of the established knowledge in their fields, but bold, more or less speculative, untested ideas connecting their very specialized work with politics, ethics, *Weltanschauung* and sciences different from their own. It is hoped that this Journal will inspire attempts to be more explicit with respect to these background beliefs, and, when formulating them, to utilize the vast resources of the history of philosophy and to look for possibilities of testing them by available research techniques.

One of the main contributions to interdisciplinary thinking in our century has been concerned with the status of statements saying that this or that ought to be, that it is valuable or that it must be. In Scandinavia Swedish thinkers particularly have broadened and deepened the philosophical debate on these questions (Hägerström, Phalén and others.)

On the whole, this debate has strengthened the tendency to avoid entanglement in normative disciplines, normative ethics, for example, and to conceive philosophical ethics as meta-ethics. There is room here for reorientation without giving up important contributions to meta-ethics.

The stress on "Wertfreiheit der Wissenschaft" (Weber) helped the researcher to resist the attempts of politicians and churches to attach normative claims to his non-normative sentences and to adopt them for purposes of propaganda. But it also promoted the tendency of researchers to withdraw from highly controversial, normative questions. The opinion was strengthened that the researcher "as such" cannot contribute to the solution of questions about what ought to be. But there is no reason to suppose *a priori* that there are questions of ethics or politics discussed today which are in principle beyond research, if thereby is meant that research cannot furnish results which would or could affect rational decisions. If absolutely ultimate and categorical valuations and imperatives were alone responsible for a decision one way rather than another, research would be irrelevant, otherwise not. The combination of philosophical perspective, analytical clarity and patient empirical research might contribute to the positive solution of (important contemporary) political, ethical, cultural and social questions. Efforts in this direction presup-

pose more stress on the policy aspect of philosophical inquiry and the willingness to take up questions of a complexity that defeats a narrow professional and technical approach.

It has been stated by an eminent scientist that science can only tell us how to make an atom bomb, it cannot help us to decide whether it *ought to be* dropped. But the kinds of arguments which are discussed in connection with the question of whether it ought to be dropped are largely questions of research: the various social sciences, psychology, history and also ethics, logic and other disciplines are involved. The distinction between a world of fact and a world of value is often misapplied when used to limit the *relevance* of research.

A normative question may not be solvable by science alone, but a non-normative one is not thus solvable either. Some postulates, whether normative or non-normative are always necessary, and the choice of postulates seems not to be entirely a question of science.

In the debate concerning the status of philosophy in relation to the more or less developed, more or less interdependent departments of research, we should like to stress the profound interdependence of purely contemplative, speculative or analytical philosophical activity dominating our philosophical faculties, and the fact-finding and interpretational activity in the special sciences.

We have witnessed during recent decades an extraordinary expansion in the volume of research activity in the behavioural and the social sciences. Not only have academic scholars increasingly resorted to direct field work and controlled observation; but there has also been a great deal of expansion in the production of official statistics and a continuous widening of the scope of systematic data gathering by means of organized efforts of various sorts. A variety of new methods of data collection have been developed, and a number of them have become generally accepted in a wide range of countries; we are thinking, in particular, of the development of extensive programs of psychological testing and of the organization of regular sample surveys and public opinions polls. All these developments, whether narrowly academic or on lines of practical application, have led to more and more extreme forms of differentiation and specialization. In particular, the exigencies and large scale data-gathering operations have tended to accelerate the division of labour and have created research bureaucracies of increasing complexity. There

has been a tendency to over-emphasize the importance of techniques and research apparatus and to lose sight of the long range problems of theoretical integration. The universities have done very little so far to counteract such tendencies; on the contrary, the rigidity of the training for the various traditional careers has tended to accentuate the importance of disciplinary role differentiation and to increase the pressure towards myopic specialization within narrow fields of technical expertness. This Journal sees a challenge in the situation. We are not asking for a return to the age when philosophers were also psychologists, anthropologists, economists and historians. We accept all the benefits of the division of labour, and we see the need for further development in the organization of programs of collective research. But we think it essential to remind the specialists of their historical ties with philosophy, of the need to go deeper into the assumptions of their inquiries and the implications of their researches, of the importance of tackling problems from a variety of angles and of cutting across the traditional boundaries of the disciplines. We are convinced that important advances in theorizing can only be made through such "boundary-crossing", and we hope to be able to stimulate philosophers no less than behavioural and social scientists to come out of their narrow fields of specialization and join in a series of "debates at the frontiers".

Research oriented towards specific problems, as opposed to research oriented towards a specific discipline, will nearly always lead to research which will cross the boundaries between disciplines, and when the questions are formulated comprehensively and clearly, they will touch foundation-problems and other problems raised and discussed by philosophical classics. We are therefore in favour of a program of training for specialists which includes philosophical classics and the use of analytical techniques, and, as well, a program of training for philosophers which includes training in conducting research in the special sciences. Mutual isolation seems destructive to both and is a major threat in our contemporary cultural and political life. The remote ideal we have in mind is that of *paideia* in ancient Greece and the research-mindedness of the pre-Socratics. There was no clear-cut line of demarcation between philosophy and the particular sciences; curiosity guided the questioner, and he was free to oscillate between the most abstract, fantastic and general considerations and considerations of minute details of individual objects, animals or men. The interconnectedness of questions

is apt to foster this oscillation. One should not let the necessity of a far-reaching division of labour obscure this fundamental interconnectedness and the consequent artificiality of many traditional distinctions.

The fields to which the Journal will be devoted are extensive and difficult. What can we do in Norway for their cultivation? Very little, indeed. It is, however, our hope that the systematic stress upon problems and our eagerness to contribute will induce some readers to take up the same question. They would thus be more extensively and intensively investigated.

NOTES ON THE DIFFERENCES BETWEEN PHYSICAL AND SOCIAL SCIENCE

by

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1. *Introduction.*

The development of the social sciences in our century has been accompanied by much thinking concerning the intrinsic nature of the social sciences, particularly as contrasted with the "older" and "more mature" physical sciences. These speculations have to some extent been of a pragmatic nature. There has been a desire to hamper the application of scientific methods to social issues. This is partly a sound reaction against over-simplifications and the tendency to jump too easily from scientific theories to broad political conclusions; and partly a fear of these modern theories and a desire to maintain a status quo. But the drive in the other direction seems to have been, or at least to be, stronger. Motivated partly by scientific curiosity and partly by reformism, the social sciences have acquired a large stock of empirical material, of theoretical constructs and theoretical connections, a methodological basis, and to some extent an institutionalization of the application of some of the fields of investigation. But the discussion concerning the intrinsic differences between the *Geisteswissenschaften* and the *Naturwissenschaften* (which does not correspond exactly to our dichotomy) continues. Much has been said about the normative character of the social sciences and the *Wertfreiheit* of the natural or physical sciences; about the possibility of measurement and application of mathematics in the two, etc. But it seems that no generally accepted conclusion has been arrived at concerning the pertinent question whether the difference is one of kind or one of degree.

It is the attitude of the present author that most of these terms which aim at dichotomizing the field of scientific inquiry and activity are sufficiently unprecise to admit the conclusion wanted, and that the conclusion drawn is more dependent upon taste than upon intersubjectively reproducible classification. However this may be, there is no doubt that the differences so often pointed out between the two fields of investigation are important, and ought to be clarified to throw light upon scientific behaviour in general, and either of the two fields in particular. In the following, ten such differences will be presented and commented upon. They will be presented partly in a dichotomous form, it being understood that one of the alternatives applies to the physical, and the other to the social sciences; and partly as an ordered set of alternatives where a dichotomy may easily be introduced by a more or less arbitrary division. It goes without saying that these are average differences, in the sense that the distributions of methodological "units" (variables, experimental settings, experimental procedures, laws, theories) from either of the two fields in the two classes provided by the dichotomies, are heavily skewed.

The important question is whether one of the dichotomies introduced may serve as a *sufficient* basis for an evaluation of the two fields, concluding with a judgment that the one is more scientific than the other. If this is answered in the negative, the next question is whether a suitably chosen set of differences may provide us with a criterion for a higher or lower level in science at large. No effort will be made to deny the rather obvious fact that theories in *physics* make use of more refined and abstract theoretical constructs, and that the level of prediction is higher than in *sociology* (the two sciences chosen to represent the two fields). Besides, we know very well that more mental energy has been and is still poured into physics than into the social sciences, though the balance is now more favourable to the latter. Either one may say that this gives sociology a serious handicap of for instance *three* centuries compared with physics, a handicap which can account for its inferiority in unification of theoretical systems and ability of prediction. Or one may look for the alleged intrinsic difference or historical background that can explain the time lag. Both views are important for the history of science, but we will make no effort to explore the historical background in order to arrive at a causal explanation for the widely held opinion that sociology is less scientific than physics.

2. The Scientific Method.

In order to have a frame of reference for our judgments, we will try to present a bird's eye view of the scientific method. We will present it in such a way as to provide a suitable point of departure for a simple, but useful typology of the differences to be presented; and in a way which is felt to be a common basis for physical and social sciences, or at least for the physical sciences.

2.1. *Scientific propositions.* The scientific procedure will here be considered as a process which has as its first aim the assertion of general implications, i. e. propositions of the logical form

(1) $(x) [P(x) \rightarrow Q(x)]$

A general implication is based upon three parts: a set X, a set of "conditions" P, and a set of "results" Q, "conditions" and "results" being conceived in a rather general sense. A proposition of this form may be said to lay down an *invariance*. The following requirements are important:

1. The proposition must be *precisely formulated*, in order to make *intersubjective communication* within a competence group possible.
2. The proposition must be *synthetic*, i. e. have empirical referents, and the conditions under which the proposition will be regarded as confirmed or disconfirmed must be indicated.
3. The proposition must be *general*, i. e. the cardinality of the set X must not be too small. X may be either extensionally or intensionally defined. Particularly valuable are sets which are "open" in the sense that they are intensionally defined, and predictions may be made concerning approved candidates for membership in the set. X must by necessity be enumerable (in order that the testing procedure may be carried out), but not necessarily finite.
4. The proposition must be *testable*, i. e., there must be given an intersubjectively communicable procedure related to the proposition, applicable to all members x_i of X. The proposition is regarded as a prediction about x_i when x_i is substituted for the variable x in (1). This singular hypothesis may be verified or falsified.
5. The proposition must be *tenable*, i. e., a sufficient number of predictions must be verified. The *degree of confirmation* in the proposition is a complex function of the number of verified predictions and the qualitatively different elements of X for which the pre-

dictions have been verified, and the number and kind of elements for which the prediction has been falsified. The testing procedure must be inter-subjectively reproducible, and there must exist a certain amount of consensus within the competence group concerning the evaluation of the degree of confirmation. If the proposition is thus regarded as tenable, it is no longer called a hypothesis, but a *law*.

Many sciences in the group which is customarily called Geisteswissenschaften are more concerned with elaborate and detailed statements concerning particular members of the set X than with general propositions. In history, more emphasis is generally put on the description and propositions concerning single events than on invariance-hypotheses about the classes whose members the elements may be considered to be. For several pragmatic reasons this presentation is natural, but it is unscientific according to our formulations above. From this conception of 'history', however, another conception may be inferred; a science of history dealing with general propositions rather than with singular interpretations of vaguely, if at all, formulated general propositions. In comparison, astronomy would lose much of its high prestige among the sciences if it were only to deal with life-histories of single planets, leaving the general law to the imagination of the reader.

The second aim of the scientific procedure is the formation of theories. Contrary to the formulation and testing of general propositions, however, this will not be considered as a *conditio sine qua non* for a science; it is a later stage, or at least another stage (see below).

2.2. *Experimental procedures.* The techniques for testing invariance-hypotheses may be labelled "experiment" or "observation". "Observation" seems to be used in two different senses; as a part of an experiment, or as a method of inquiry on par with experiment, where the conditions are given by nature or by society, and are not controllable by human manipulation. The latter difference, however, is of minor importance, and is bridged by the "ex post facto-experiment". For our purpose, they will be grouped together under the term "experiment", and the experimental procedure may be described as follows:

1. A spatio-temporal region is *selected*, either because of its alleged relevance for a hypothesis to be tested, or just for exploration.
2. The complementary set (the "conditions", the "surroundings") is in-

vestigated for its possible influence on the result of the experiment. It is divided into two parts; *the relevant and the irrelevant conditions*, where the relevant conditions have to be controlled.

3. The selected region is asymmetrically structured into two non-overlapping subsets called *independent variables and dependent variables*.
4. By some procedure *connected values* between the independent and the dependent variables may be found and
5. These connected values may be used to *confirm or disconfirm* the hypothesis or to form a *hypothesis by induction*.

This simple figure illustrates the five steps, and gives a key to many of the ten differences to be presented.

WORLD

Conditions		Variables	
Irrelevant	Relevant	Independent	Dependent

EXPERIMENT

For most experiments the irrelevant conditions will form the overwhelming part of this four-part partition of the world.

The word "value" is here taken in a very general sense which does not imply any use of numbers. What is usually called "experimental design" enters on all five stages. The four points are interrelated and the order is often more logical than temporal.

2.3. *Theory formation.* When several laws have been found concerning the same set X or concerning different, but related sets, the need for a unification of them in a common structure arises. This is part of what the theory formation tries to achieve. Another part consists in the effort to provide a basis for the inference of new invariance-hypotheses which may in turn be tested. A theory which is successful in both respects is said to be fruitful (not tenable or true) and is often presented as a hypothetico-deductive system.

This division of the scientific process in experimental procedures and theory formation will serve as a point of departure. That these fields are interrelated is obvious. They may be conceived as a more or less arbitrary cut along the dimension of alienation from the sense-impressions.

3. Ten differences reviewed.

The number of differences reviewed, i. e. ten, is admittedly arbitrary. They all refer to important aspects of the methodology of science, but the number could easily be enlarged or condensed. They are divided according to the division presented above in differences related to experimental procedures and differences related to theory formation, but no further attempt will be made towards a classification. Neither is the classification unique, (D_1 and D_6 are particularly doubtful) but it is believed to be more valuable than no classification at all.

3.1. Differences related to experimental procedures.

D_1 : Measurable vs. non-measurable variables.

We have made "variable" a basic term in the scientific language, it being understood that it comprises all sorts of entities that represent the elements of a set. The equivalence classes in a set according to any equivalence relation correspond to what we call "values", and the value of an element is found by mapping that element on its equivalence class.

By "measurement" we mean a 1,1-mapping of the values of a variable on (or in) a set of numbers. This mapping may be more or less arbitrary, and referring to the kind and degree of arbitrariness, we introduce what is known as the four principal scales of measurement.

Nominal scale, where the mapping is 1,1 (just ordinary classification).

Ordinal scale, where there exists an asymmetric and transitive relation in the set of values such that the values may be ordered, and the measurement mapping preserves this order.

Interval scale, where the mapping is made so as to give empirical meaning to the equality of the differences found between two pairs of numbers.

Ratio scale, where the mapping is made so as to give empirical meaning to the equality of the ratios found between two pairs of numbers.

If by "measurable" we mean a variable which is measured in interval or ratio scale, there is little doubt that the dichotomy measur-

able-nonmeasurable constitutes a difference. Many of the basic physical variables are measured by ratio (the dimensions L, M, and T) or interval scales (temperature in degrees Celsius), which in turn has made it possible to apply mathematics to an extent not equalled in the social sciences.

But first of all it must not be forgotten that many important physical variables, as indices of atomic quantities, etc., are not made measurable till now. And secondly — the whole concept of measurability is formed to suit physical needs. The definition of measurability is of course to some extent arbitrary, and though it serves excellently the needs of physics, we may not infer that it will ever serve the needs of the social sciences. For one reason: the transition from nominal to ordinal scale depends upon a relation with certain properties; such relations are rarely found in the social sciences. If the relations found introduce an order, it is more likely to be a partial order. And the transition from ordinal to interval scale through the application of "scaling" is a rather doubtful one. In any case, "measurability" does not make a science more scientific than another, if it does not enter into some basic definitions of "science". And, there are many other parts of mathematics, admittedly to a large extent still to be developed, where the social sciences may look for abstract structures which can be applied. The problem of increasing the level of measurement will always be an important one, but rather than to try to fit social data into one of the strong scales and arrive at uninteresting conclusions, one ought to try to find other formal structures which fit the data better; perhaps structures which are formally less rich, but still may give more interesting conclusions. The problem of measurement seems to have been haunting the social scientists to excess, and may perhaps be looked upon as a sign of an inferiority complex and imitation of the older sciences.

The fundamental definition of measurement given above may be retained, but new scales may be introduced. Numbers have very rich algebraic properties, and the problem is that of making use of just the properties which give an isomorphic mapping with social science data. Or, one might leave the numbers and turn to other algebraic systems such as posets, lattices, different groups etc.

D₂: Controllable vs. non-controllable experiments.

The experimental *conditions*, as we have defined them, constitute that part of the world which is not included in the set of dependent

or independent variables, and the *relevant* conditions are those which are believed to influence the result of the experiment conducted. Now, if the experiment is to reveal connections between independent and dependent variables, a necessary condition is that the relevant conditions are controlled. This is usually done in two ways, where the second is a special case of the first.

1. The relevant condition is controlled by keeping it at a constant value during the experiment. If the experiment is repeated, the condition is kept at the same value.
2. The relevant condition is controlled by making it irrelevant, i. e. by giving it at zero value. If the experiment is repeated, effort is made that the condition is still irrelevant.

By this procedure, we are standardizing the conditions under which the experiment is conducted, to make the repetitions comparable.

Now, it may happen that a relevant condition, or a set of relevant conditions is not controllable in either of the two classical ways stated. Or it may even happen that some or all of the relevant conditions are not known, or more correctly, are known not to be known. In these cases, the experiments are said to be non-controllable.

There can be little doubt that the dichotomy controllable-non-controllable experiments constitutes a difference in our sense. In the social sciences, we very often know that the set of relevant conditions is big, complex, interwoven, but we do not know how to control. The usual matching techniques, for instance, are very difficult to carry out, at least if we are to control the most relevant conditions. We cannot eradicate completely the influence of a person's past even for a short time, for technical reasons. And we *cannot* keep human beings under extraordinary strain, even though it might be scientifically most valuable, for ethical reasons. And in the case of interviewing, for instance, we know very well that the interviewer himself is a most relevant condition, and it is even relevant for the result whether the interviewee is no. 1, no. 25 or no. 50 on his list of objects.

If there was no possibility available for the control of such non-controllable experiments, it would be rather detrimental to the social sciences. But a technique has been found, particularly due to the influence of R. A. Fisher — viz. *randomization*. It may be included in the list above as

3. The relevant condition is controlled by randomization, i. e., the random distribution of the experimental units among the (known or unknown) values of the condition, so as to prevent the condition from making a systematic influence on the results.

These and other techniques from the rapidly developing field of experimental designs (particularly the analysis of variance) have made experimental control possible to a great extent. Without the two first types of control the variability in the experimental data increases quite considerably, and the probability of making a type II error in the statistical language increases correspondingly as it gets more difficult to obtain significant values. And the problem of more exact control methods is still an important one. But we may conclude by stating that this is also the problem of many physical experiments, — or for instance agricultural experiments. Without the third way of controlling “non-controllable” experiments, the social sciences would have been far worse off. Today, D_2 amounts to little more than to say that the classical physical text-book experiment is of little use in the social sciences, whereas the more refined statistical techniques and experimental designs find their use.

D_3 : Determinate vs. stochastic experiments.

We have an experiment which is controlled according to one or a combination of the principles presented in the last paragraph. The independent values are as far as possible given determinate values, the experiment is conducted and the value(s) of the dependent variables found. As examples, we take ball-rolling down an inclined plane, coin-flipping, and interviewing.

	Ball-rolling	Coin-flipping	Interviewing
Independent variables	mass of the ball <i>inclination of the plane</i> coefficient of moving friction ball-plane.	<i>bias of the coin</i> , flipping conditions, force applied, twist given etc. motion in the air, adhesion floor-coin	<i>question asked</i> person interviewed way of asking, intonation etc. interviewer used
Dependent variables	<i>distance rolled</i> time needed to roll a distance	<i>face shown</i> number of turns after having touched the floor place fallen	<i>answer given</i> emotional reactions time needed for reflection etc.

Some of the independent variables may very well be included in the set of relevant conditions and be controlled. In the classical experimental design in physics only one of the independent variables is varied at the time, and the others are kept at constant values, i. e., they may be said to be included in the conditions of the experiment. The distinction between "condition" and "independent variable" is, however, pragmatic and not intrinsic to the experiment.

In an experiment where

1. the relevant conditions are controlled as far as possible,
2. the independent variables are given fixed values as far as possible,
3. the experiment is conducted and the value(s) of the dependent variables found, and
4. the experiment is repeated under as far as possible the same conditions, and values of the independent variables,

two things may happen. Either

1. repetitions yield the same values of the dependent variables, or
2. repetitions yield different values of the dependent variables.

In the first case the experiment is called "*determinate*", in the second case it is called "*stochastic*". Some qualifications must be made, however. Firstly, one ought to distinguish between determinate or stochastic for one particular set of values of the independent variables, for the range of values, or for all possible values. The distinction depends upon the exact definition of the experiment conducted, whether it relates to a singular set, a range, or to all possible values. Secondly, whilst an experiment is said to be stochastic as soon as a repetition has yielded a result differing from other results, we infer that an experiment is determinate from the knowledge of a finite series of repetitions. The hypothesis that "an experiment E is determinate" is thus easier to reject than to accept; the problem is analogous to the general problem of induction. In our decision concerning E, our knowledge or hypotheses concerning E will enter. And thirdly, it is understood that "the same values" is to be interpreted as "the same values within the limits of measurement". This makes it to some extent conventional what we will regard as determinate or stochastic. If the dependent variable is continuous, "the same values" must be interpreted as "the same range of values", otherwise the definition would be operationally void.

The dichotomy undoubtedly does constitute a difference in our sense. For the same reasons as stated in the last paragraph, stochastic experiments are more frequent in the social sciences than in the physical ones. We rarely manage to keep at a constant value both conditions and independent variables, and a variability in the values of the dependent variables may result. We may by definition use the presence of stochasticity as a sufficient condition (though not necessary) for the absence of what we may call complete control and fixation, and use stochasticity to indicate that we have not managed to control or fix the independent variables completely. But further control may be impossible for technical, economical, or ethical reasons, or simply not wanted for theoretical reasons. Further control may distort the natural setting and create artificial and theoretically insignificant situations.

The prevalence of stochasticity in social science experiments has another important methodological implication. Without entering into the discussion of the implications of the Heraclitian dictum *panta rei* on the possibility of performing an *experimentum crucis* (depending on a repetition) in science at large, it may safely be said that such experiments are less performable in the social sciences than in the physical ones. Conditions will vary, and will be known to vary, too much from one repetition to another. Here, we may also seek the reason why the problems of *reliability*, both of the observer and the observed, are so important in the social sciences.

But is the difference in any sense detrimental to the social sciences? It makes it often impossible to predict or predicate anything concerning all members of a given class, for instance about all members of a social group. But, thanks to the statistical techniques invented in the last generations, we are not at a loss. Instead of predicating about elements, we may predicate about samples from a specified universe. We may be able to state that out of a sample of 100 youngsters selected at random from a certain district in Oslo, 15 ± 5 will commit a crime before they are 25 years old. Of course, the effort to find variables or combinations of variables which are fundamental in the sense that they make exact, i. e., determinate, prediction possible, will always be important in all scientific behaviour. But the statistical prediction is not contrary to any of the necessary conditions for a knowledge-seeking process to be scientific. Finally, it may be added that

statistical techniques and probability statements play an increasing role in modern physics, as evidenced by the theories of quantum mechanics and statistical mechanics.

D₄: Projected vs. Ex post facto-experiments.

We have in preceding paragraphs tacitly assumed a model of the experimental procedure where the experimenter is deliberately choosing values of the independent variable(s), and watching what happens. This model, however, does not account very well for the way many, perhaps most, experiments in social sciences are conducted. Typical of these "experiments", and we may call them so according to our definition in part 2, is that the experimenter arrives at the experimental scene after the experiment has been conducted, and that he tries to trace the way it proceeded. He arrives Post facto.

The background is that technical, economical, ethical or theoretical reasons very often prevent the experimenter from undertaking the manipulations he would have liked to do *in order to perform a projected experiment*. Particularly important are the ethical and theoretical constraints on his behaviour. One cannot order a random sample to live for five years in slum conditions or under other frustrating conditions to see whether aggressive behaviour ensues. And, because of the objects' knowledge that they participate in an experiment, projected experimentation always introduces an element of artificiality and methodological difficulty (D₆ and D₁₀).

There can be little doubt that the dichotomy constitutes a difference in the sense stated. And even this intrinsic difference would have been a rather serious one, if it had not been for the invention of ex post facto techniques. True, the ex post facto experiment is also an experiment, even a projected experiment. But instead of manipulating the objects, e.g. the human beings, symbols for them, such as punched cards or paper-and-pencil-symbols are manipulated. The crucial point is the transition from different sources to the symbols, by means of interviews, questionnaires, written sources etc. This is the weak point. When the punched cards are available and punched, the rest is a matter of usual techniques and ingenuity where the direct control of the projected experiment is substituted by the indirect control of the ex post facto experiment.

D₃: Degree of interaction between the observer and the observed.

The observation itself or other parts of the experimental procedure must always be regarded as elements in the set of experimental conditions. The salient question is whether they are relevant conditions or not. It seems to be a part of the philosophy of experiments that the experimental process itself ought to be an irrelevant condition for the experiment, that is, the results found shall have bearings upon the phenomenon studied even in a "natural" setting without any artificial, experimental manipulations. Now, this view is hardly tenable. The artificiality created by the projected experimental setting, where events happen which would ordinarily not happen, or at least not happen in exactly the manner prescribed by the experimenter, is often a necessary part of the analytical procedure which may give us more knowledge concerning the nature of the phenomenon. The question is whether the phenomenon is distorted in any way by the experiment so as to invalidate the conclusions drawn if they pretend to cover a more general class than the experiments only.

It is well known that the so-called Indeterminacy Principle of quantum mechanics concerns an interaction between the observer and the observed. The experimental procedure interferes with the phenomenon studied (i. e. the simultaneous position and velocity of an electron) in such a way that the observation is not possible — it is not operationally defined. In this connection we have a conception of the nature of the phenomenon without the experimental interference; a conception which in this case is derived from macrocosmic phenomena and projected into microcosmos. In the same sense, we have a feeling, an intuition about social phenomena, derived from common-sense experience. When social phenomena are studied experimentally we feel that deviations from the normal development of social phenomena will inevitably occur.

The observer and the observed are all human beings, and they will always constitute a social system during the performance of the experiment. Characteristic of the social system is the *interaction* (the double contingency) between its members. Now, the degree of interaction related to the experiment may be reduced by artificial devices, one-way screens, tape-recorders etc. But these devices are easily recog-

nized by the objects and acquire a symbolic value as substitutes for the experimenter. The artificiality of the setting is easily perceived if the object is not particularly trained to ignore it.

Another way of controlling the experimental procedures as relevant condition is to conceal the experimental nature of the setting. *Participant observation*, without the objects' knowledge, is one method — applied in factories, prisons and other institutions. Projective techniques are even more important. Here, the object may well be aware of the fact that he is interviewed or investigated, but the hidden intention behind the questions is not revealed to him — he does not know that the interviewer will not take his answers at their face value, as he pretends to do with his nodding acquiescence and encouraging remarks.

The difficulty with these techniques is not that they are inefficient; they undoubtedly work to some extent. But from an ethical point of view, they are rather dubious. What right does the social scientist have to "investigate" his fellow beings, and where is the borderline between scientific objectivity and simple, common-sense curiosity? What guarantees can scientists give their objects? And to what extent can he comfort himself with their willingness to cooperate, as long as the objects themselves do not clearly see the implications of his investigations?

This is particularly important in all kinds of research which has bearing upon more or less burning social issues. The subjects interviewed or in other ways investigated may foresee some of the conclusions, and give their answers a bias so as to provide for the wanted conclusion. Of course, there are ways to correct for this, but still it is a problem. In prison research for instance, two important difficulties experienced by the present author were the prisoners' overwillingness to cooperate, and the guards' rather reluctant attitude toward the whole idea of an investigation. To the question: "What do you think society hopes to achieve by putting convicted people into prison?" the almost unanimous answer from the prisoners was an emphatic "nothing". On the probe: "Well, but what do they *hope* to achieve?" — the simple logic of the question did not penetrate through the eagerness to find an outlet for aggression, and the answer was still most of the time "nothing". The question had to be restated several times before more differentiated (but less expressive) answers were obtained.

There seems to be little doubt that the often inescapable inter-

action between the observer and the observed in a social science investigation constitutes a very serious methodological difficulty. To lower the degree of interaction is technically difficult, and to change the content of the interaction by pretending that it does not concern observation is ethically dubious. No general and intersubjectively reproducible way of correcting for the effect of the observation interaction is known to the present author, except that of "thinking the effect away".

3. 2. *Differences related to theory formation.*

D₆: Causal connection vs. interaction and covariation.

Among the different ways of presenting invariances found by means of the experimental method the causal law has always had, and has still, a leading position. Though Wittgenstein holds that "The Law of Causality is no law, it is the form of a law", statements on causality seem to have higher prestige than other ways of stating the relationship between independent and dependent variables. If we interpret "A is the cause of B and B is the effect of A" as "B is constantly succeeding A" we get a useful formula. However, a descriptive definition of causality seems to be much more complex. Besides, the domineering role of the causal relationship seems partly to be due to pragmatic reasons. The knowledge of a causal law gives us power over the phenomenon if it is stated that A is a necessary and sufficient cause of B, or at least that A is a sufficient cause of B.

Interaction and covariation are other ways of stating a relationship. They may be said to be less pretentious and less penetrating and at any rate less interesting from a pragmatic point of view; but still they are a way of giving invariances, and thus as scientific as the causal relations. It seems that the dichotomy does constitute a difference; i. e., that we in the social sciences find more cases where the theories are presented in terms of interaction or covariation, though an interpretation in causal terms is desirable.

The reasons for this are many. First of all, through our knowledge of the social world thanks to our *Einfühlung* and introspection, we get an intuitive feeling of the enormous complexity of the connections between the variables — and this feeling may prevent us from further efforts to find necessary and sufficient causes for an effect.

Besides there are the usual reasons which prevent us from a penetrating research; technical, economical, ethical reasons. Thus we often satisfy ourselves with a statement, most often in stochastic terms, of co-variation between crime rates and degree of brokenness in the home, without the pretension that the latter is a necessary and sufficient cause.

There is, however, another reason which is perhaps more important. Social systems are systems of interaction, where there is mutual dependence and not the one-way dependence so typical for the causal relationship. Such mutual dependences may also be found in physics, viz., in the theory of servo-mechanisms which seems to be, or already is, a very promising field from which the social sciences, particularly sociology, may borrow analogies and models. The temporal asymmetry so typical for the causal relationship cannot be provided for by the analysis in terms of interactions; interaction is not merely a juxtaposition of two causal relationships.

The difference is hardly felt to be serious for the social sciences, except by scientists or philosophers who still believe in the causal way of presenting relationship as *the* scientific form of a proposition. Relationships believed to be causal are in the social sciences usually found by some application or another of the famous Method of Differences from Mill's *A System of Logic* (but usually restated in the converse form: "If anything C is to be the cause of the effect E, it must occur when E occurs and not occur when E does not occur," or the contrapositive of this statement.) Because of the frequent stochastic nature of social science experiments, an experiment modelled according to the Method of Differences has to be replicated. Such an experiment, however, always implies the control in one way or another of n variables or relevant conditions, where n may be a large number. The salient question seems to be, whether more insight in social science matters will make n increase or decrease in our conception of the setting of the experiment. One may argue both ways: Further progress will enable us to "see" one or a few relevant variables more crucial than we could imagine before, and hence to design experiments with better control. Or further insight will just give us insight in a larger number of relevant variables. Presumably, both views are tenable simultaneously. Probably, it will not be more feasible to present causal relationships if one knows a few very important variables, when at the

same time n has doubled or trebled. And the dilemma for the social scientist will be more intensely felt when the growth of his field causes an urge from "society" to present invariances in a pragmatic, i. e. causal form.

D₇: Degree of Einfühlung.

In spite of trends like behaviorism and operationalism it is obvious that the scientist's attitude to the objects of investigation is highly different when we turn from the physical sciences to the social sciences. We usually do not endow the non-vivid nature with capacities such as perception and reflection, but feel more or less related to the objects of a social investigation. This makes a cut between the physical and social sciences rather evident; in the former what we might call Einfühlung (empathy) seems impossible, in the latter it is not only possible, but an inevitable and it seems necessary part of the scientific procedure. It seems that the Einfühling enters at one particular stage in the process: the formation of hypothesis, where techniques like introspection, the scientist's role-playing etc. are employed.

There seem to be two sides of this difference. Firstly, the scientist's capacity of Einfühlung with his objects should, compared with his relation to physical objects, make the formation of hypotheses easier, and the hypotheses by far more prolific. The zoologist is in a position somewhere in between. He is accused of anthropomorphism when he makes use of introspection or related techniques, but does so all the same. If however, he acquires some extraordinary insight into the ways animals feel about particular settings, not by experimental techniques but by other means, he would be immensely aided in his hypothesis-making, *even though no reference to his insight is made by him.*

But on the other hand, this very insight and Einfühlung makes the shortcomings of the theories formed all too evident. The objects of the theories, to a large extent human beings, may protest against it and claim that "this is only theory. The real world is so much more complicated, and you cannot describe or explain human phenomena by means of some simple formulae or sentences, they are much too complicated". This protest is always important, though negative, and may not in itself reveal any intrinsic difference between physical and social sciences. If the balls, planes, wires etc., which are the objects of the physicist's theories had the capacity to protest, they

would perhaps have made the same claim: we are much more complicated than you believe us to be. More serious, however, is that the scientist's capacity of *Einfühlung* makes him feel that the scientific process entails a Sisyphus work when applied to social phenomena, and this pessimistic attitude may prevent him from acquiring the mental activity and attitude typical of the scientist.

The problem of *validity*, so important in the social sciences, may be mentioned in this connection. The *Einfühlung* and the many connotations it gives to words and expressions from the language of daily intercourse make the scientific concepts seem barren and too restricted — and it is often all too obvious that the observations or measurements do not cover those phenomena or all the connotations connected with the word they pretend to cover.

We have preferred to talk about *degree* of *Einfühlung* in order to do justice to the wide spectrum where the attitudes of the theoretical physicist and the artist or the musician are at the extreme ends. Whether *Einfühlung* is impossible or not for the physicist is at least partly a matter of exact definition. If *Einfühlung* is explicated as the "possibility of inside control" it seems difficult to deny that this mental process takes part in the physicist — after all, he too is a part of the physical world he is exploring. But, he has other means of "outside" control which are more easily intersubjectively reproducible.

One of the methodological problems implied by the higher degree of *Einfühlung* in the social sciences seems to be this: The often apparently high degree of *Einfühlung*, combined with conspicuous shortcomings of other techniques (as statistical analysis of observed phenomena), often clear the way for the scientific description and explanation of social phenomena. The difficulty lies in the lack of intersubjective communicability of the propositions conceived and reproducibility of the mental operations for testing the hypotheses. It seems to be a most important and positive fact that *Einfühlung* was once supplied by other means of testing hypotheses, even in the social field. But the reaction was too exaggerated; today the important task for social science philosophers seems to be to restore *Einfühlung* to its deserved position in the scientific process.

There is one particular aspect of the problem of *Einfühlung* or "ego-involvement" which is worth consideration, namely the difference between criteria of the competence group of the physical scientist vs.

that of the social scientist. It seems that the social scientist needs the capacity of *Einfühlung* his colleague in physics does not need — and this capacity is perhaps not a matter of academic training. It draws from various sources — human experience, intuition, from literature etc. The intellectual training of a physicist, in mathematics and theoretical physics for instance, is partly replaced by a more soft, not deliberate, but equally necessary training for the social scientist. As the latter integrates more parts of his personality and culture in his scientific ego, the risk of a too homogeneous competence group is perhaps less in the social sciences. On the other hand, however, the risk of being equally influenced by the truisms and cultural trends of the time and the place they live is perhaps greater — thus an intersubjective reproducibility arises which becomes the subject of the ridicule of later generations.

D₈: Degree of *Wertfreiheit*.

All research is undertaken within the framework set by a society. As a participant member of his society, the scientist has a system of values and norms which more or less conforms with the dominant or official system in that society. His scientific activity is also social activity as far as he wants to publish his results or to make use of public money or to be financed from other sources. This ties him to the social system as a whole or to a subsystem, and his scientific activity cannot be carried out completely independent of the values and norms of that system and subsystem. Though he may live in a system where “free research” is a value, perhaps even an autotelic value, he is still not free in the sense that his actions are not normatively regulated. He may, for instance not deviate from the norm of free research.

There are many ways in which this linkage to a social system interferes with his research, consciously or unconsciously, internalized or not. The selection of topics for investigation, the methodological outlook, the extent of the research as regulated by financial means, the conclusions wanted or unwanted by the researcher himself, the subsystem or the social system at large. The salient question is to what extent this important feature in the sociology of research has different implications for physical and for social sciences.

It seems obvious that the implications for the social sciences are

much more severe and that much more *Wertfreiheit* may be found in the physical sciences. The subject matter in the social sciences borders on the topics Man has always been discussing, and with most divided opinions. Values are attached to them, and these values are partly internalized so that strong emotions may be stirred by the mere thought of scientifically investigating such topics. In this connection, it is strange to note that the social sciences have developed far more rapidly in the comparatively problem-free countries in Northern Europe, than for instance in Italy, where parts of the country are declared to be "underdeveloped".

Social science theories are not easily integrated in the political framework, and particularly not so when they concern the most emotional political problems where the decision-making process is institutionalized around cathectic and evaluative rather than cognitive standards.

Of course, there is not only the problem of the influence on research of *social* norms and values. The researcher's own values may be more detrimental to the scientific procedure. For a pacifist psychologist for instance, it will be rather difficult to arrive at a result which implies that war or physical violence in general is a manifestation of an ineradicable drive in the human nature. The criterion of intersubjectivity, however, is introduced in the methodology of science, among other reasons to correct for the influence of values. By careful selection of interviewers with antagonistic viewpoints on the topic under investigation, one may assess the influence of the interviewer's standpoint on the result reported. But, whereas results of interviews may be comparable, it is more difficult to compare theories and assess the relative degree of influence from the norms and values held by the theorists. Between marxist and Western theories in the social sciences there seems at present to be an unbridgeable gulf. Both parts claim to have realized the ideal of *Wertfreiheit*, and accuse the other of value-involvement. But the frames of reference and the topics selected for investigation are so different that exact comparison seems difficult if not impossible.

D₀: Degree of formalization in the syntax language.

To provide for intersubjective communication concerning experi-

ments and theory formation, the sense-impressions and the theories about them are stated in a language. We will call this language in which we talk about the sense-impressions and with which we form our theories the *syntax-language*. It is obvious that the *semantical* problem, i. e. the problem concerning the relations between what we intend to express and the linguistic expressions employed is a basic one in all science, both the physical and the social ones.

The language employed may be a natural language (e.g. English) or an artificial language (e.g. mathematics) or both, or (in most cases) a natural language heavily loaded with technical terms. The salient feature of the languages in this connection is their degree of formalization, i. e. to what extent the rules of inference are stated explicitly. In mathematics and logistics these rules are, if not always explicit, understood inside the competence group so as to provide a basis for parsimony. Given the same set of postulates, all or nearly all skilled members of the competence groups will be able to derive the same sets of theorems or propositions. This is perhaps the most important aspect of mathematics from a methodological point of view. The symbolization is not that important, though it implies an enormous economy of thought.

The natural languages may also be considered as a calculus. Given a set of postulates in English concerning some phenomenon studied, propositions may be inferred. But, usually, not unambiguously. Difficulties of interpretation, different usage of words and phrases, different evaluation of the facts referred to, different frames of reference from which the postulates are viewed will account for severe deviations from the ideal of parsimony. To make the deduction or inference less ambiguous, technical terms are introduced. They also serve the purpose of being symbols for highly abstract concepts not symbolized in ordinary language; but more important is the fact (or hypothesis) that fewer connotations attach to these technical terms than to their explications in common-sense terms.

There are very good reasons why it has not been possible yet, to any extent, to "apply" mathematics in the realm of the social sciences, i. e. to use mathematics (partly) as a syntax language in the study of social phenomena or our sense-impressions of the phenomena. Now, the entire field of mathematics may be structured by means of a four-fold table after the usual dichotomies "quantitative-non-quantitative"

and "determinate-stochastic"; where the latter corresponds to D_3 , and the former to D_1 when "quantitative" is interpreted as "measurable" in our sense of the word.

	Determinate	Stochastic
Quantitative	Arithmetic I Calculus Theory of Functions	Mathematical statistics II (theory of distribution functions)
Non-quantitative	Algebra (modern III or abstract). Topology Geometry Logistics	Non-parametric statistics IV Theory of Games

The classification is exhaustive, though not unambiguous; but the conjunction predicated for each item classified gives some information concerning the properties of that part of mathematics. There are, however, parts of the disciplines indicated which are border-line cases, but for our purpose this coarse classification of mathematical disciplines on a methodological basis is useful.

Our conclusion is derived from two premisses: Firstly, most of the data of classical physics (except statistical mechanics) are quadrant I data, or at least not quadrant IV data — and most of the data of the social sciences seem to be quadrant IV data (according to D_1 and D_3) or at least not quadrant I data. Secondly, the mental energy of mathematicians has mostly been devoted to develop quadrant I mathematics, to a great extent because of the close cooperation with physics, mechanics and astronomy — and far less to quadrant IV mathematics. Even the mathematics of quadrants II and III is a rather recent invention — apart from geometry they have been developed during the last century. Conclusion: The mathematics of the social sciences has still to be developed — and is being created. A necessary condition for the growth of quadrant IV mathematics is a closer cooperation between social scientists and mathematicians than has hitherto been the

case. Of course, the social sciences may even, and in fact do, benefit from the use of mathematical structures from the other quadrants.

D₁₀: Degree of interaction between the theory and the observed.

A published scientific theory and scientific activity as a whole, are cultural products, and elements in the cultural system. They are parts of the culture forming our patterns and providing us with the means of communication. The fact that a theory in the field of physics is published is usually not regarded as a relevant condition for the result of physical processes. On the contrary, we do not ask the dead nature what it thinks about our theories. And if we ask, we will hardly get any answer.

In the social sphere however, the publication of a theory on social phenomena becomes a new factor or condition, or even an independent variable. This has many implications. Firstly, the social scientist gets his theory commented upon by the denotata of many of the theoretical concepts. And secondly, the theory may influence their behaviour, particularly if it concerns an emotionally loaded field. *A prophecy may be self-fulfilling or self-denying*, as for instance Robert Merton so eloquently has shown. The marxist theories may perhaps partly be said to belong to the self-denying prophecies in the Western world. They have certainly not come true, but part of the reasons for their failure as predictions may be found in their publication and its social consequences.

Another example is given by the behaviour which psychologists and students of psychology seem to be prone to, at least according to the stereotypes. The frustration-aggression hypothesis for instance, may conceivably change a person's behaviour when it is internalized (as by a student of psychology) and lead to an (unconscious) fulfilling or denial of it according to whether it is accepted or rejected by the student.

There is however a third effect of the formation and the publication of social theories on the observed objects which is most important. Social consequences are drawn from the theories and applied in the political field. If the theory is pragmatically successful in the sense that the results of the applications conform to common values, the theory gradually acquires a normative character. It is transcribed in the lan-

guage of "ought" instead of the language of "is", and may become more or less a part of the social code of that society. This may have no dangerous implications for the scientific value of the theory if the distinction between the normative and descriptive aspects of the theory is clear. But there will always be a danger that falsifications of predictions from the theory are hidden or explained away because of the theory's value as a doctrine, and that modification of the basic hypothesis is not only prohibited, but even inhibited.

A fourth important implication of the interaction between the theory and the observed is this: The observer or scientist may so much want his theory to be fruitful and its predictions to be true because of scientific ambition, political prestige or for pure esthetic reasons, that his theory even for him slides on the continuum between the pure descriptive and the pure normative, and towards the normative end of it. If he has no power to enforce his theory, this may be detrimental to no one except himself; but if he has, it may result in one kind of authoritarianism or another. Whether the theory is scientifically a masterpiece or just diletantism of the kind our century so often has witnessed, makes no difference to the victims who do not live according to the rule. On the contrary, the more perfect a social science theory is, the more dangerous may it be when it acquires normative value and is imposed upon others. There is hardly anything similar to this interplay between theory, observer and the observed in the physical sciences — as we do not interact with the non-social objects we cannot change their behaviour in this way.

Finally, once a social theory is published, a new situation is created as mentioned above. This may make the criterion of intersubjective reproducibility almost void, as the conditions of reproducibility are not present. For social research to be comparative it seems to be a necessary condition that it is carried out simultaneously, and not in succession. Methodologically, however, simultaneous reproducibility may be as useful as the successional one for the intersubjective checking.

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Other "differences" than these ten have been considered, but rejected. There are numerous applications of the proposition function "in the physical sciences we have x , whereas in the social sciences we have y ", and few restrictions seem to have been placed on the selection of x

and y . The reasons for rejection have been two: Either the alleged difference seems to be subsumable under $D_1 \vee D_2 \vee \dots \vee D_{10}$ — or it seems to be no difference at all. Examples of rejections of the first kind are differences referring to different degrees of reliability and validity in the two fields. Examples of the second kind are the alleged normative character of social science laws, a difference that seems to be rooted in the apparent ambiguity of the word “law”. That invariances in the social sciences may be apprehended as norms does not alter their descriptive character.

Another example is the old MacIver difference:

“There is an essential difference from the standpoint of causation, between a paper flying before the wind and a man flying from a pursuing crowd. The paper knows no fear and the wind no hate, but without fear and hate the man would not fly nor the crowd pursue”. This is undoubtedly true, but the importance of this difference seems to have faded. It has to do with D_7 , and the arguments used apply to some extent to this case.

Finally, there is the old saying that “social phenomena are more complex than physical phenomena”. If we agree that complexity is not an intrinsic quality (whatever we might mean by this!) but a property projected into the phenomena by the observer and a function of his habits and his insight, the “difference” seems to be soluble. The complexity may be regarded as being due to our capacity of *Einfühlung* whilst at the same time we lack scientific insight — and thus reverses the situation we are used to when we look at the physical sciences. As such the difference has to do with D_7 and D_{2-3} .

4. *A difference in kind or in degree?*

If we look through the list, and construct a ten-dimensional space where methodological units or sciences may be mapped as points depending on their values on the ten difference-coordinates $D_1 \dots D_{10}$, the distance between the physical and the social sciences will be substantial. We may orient the axes so as to place Newtonian mechanics — (with its measurable variables, its controllable and determinate experiments, if necessary projected, with no known interaction whether between the observer and the observed nor between the theory and the observed, with the theory stated in causal terms and in a mathe-

mathematical language with unambiguously deducible propositions and (at least to-day) no bewildering discussions concerning amount and kind of *Einfühlung* and lack of *Wertfreiheit*) — in the corner that includes *origo*. In the opposite extreme we will find for instance the typical survey experiment. As the social sciences entered the scene *after* the concept of Science had been moulded according to the needs and the peculiarities of the physical science, there is no reason to wonder that this methodological difference was too great to make sociology look acceptable. Time was needed to expand the concept of science so as to include even the social sciences.

If we take the differences one by one, we will find that they can all be found in the history of science, and even before the social sciences made their arrival. The biological sciences, for instance, are in an intermediate position. All the differences except D_{10} seem to play some methodological role, if not to the same extent as they do in the social sciences. D_8 for instance implies difficulties which are evidenced by the life history of Charles Darwin and the anti-vivisectionist leagues. D_{2-3} are very important, and their implications may be judged by the fact that the founder of the bulk of modern statistics, R. A. Fisher, was working in these sciences.

In this space, a precise meaning, if any, may be given to the expression "The science S is a soft science". Degree of softness may be measured by the distance from *origo* and the angle the radius vector to the point representing S makes with the coordinate axes. But which axis is the most important one, in measuring softness? And how do we feel sure that there is not a contradiction between the statements " S is a science" and " S is mapped as a point x in the space, where x is an arbitrary point in the space", for all points x ?

This essential question must find its answer by a careful scrutiny of the compatibility of the definitions of science given in the second part and the differences presented. We have given some items in this discussion and will sum up in a more sweeping statement. D_1 does not interfere with the scientific process, as "measurability" is a concept partly derived from the nature of the physical data — and other mathematical structures than numbers are and will be available so as to provide for the access to mathematics as a syntax language even for the social sciences (D_9). Randomization, experimental design and statistics are the solutions of the problems implied by D_2 and D_3 . Symbolic

manipulation and indirect control solves to some extent the problem implied by D_4 , though there are and will always be enormous semantical difficulties. D_6 is partly rejected as a problem with the motivation that the prestige of causal relations is partly based on a habit from the physical sciences, partly because of the prevalence of interaction analysis; and partly accepted as involving an experimental difficulty for the social sciences linked together with D_{2+3} . D_7 ought to be regarded rather as a plus for the social sciences than as a minus if any comparison is to be made. D_8 concerns a social problem what the society's interference is concerned, and (probably) a question of tradition and training what the scientist's internal conflicts are concerned.

D_9 and D_{10} are left. If we concentrate on the methodological implications of D_{10} , it seems undeniable that some possibilities of methodologically important reproductions of experiments are distorted or completely disturbed by publication. In principle, however, this may always be avoided if science is institutionalized in such a way as to make a deliberate retention of theories natural.

Finally — we arrive at D_5 . Parsons* mentions the famous mathematician Norbert Wiener who considers "that this is a fundamental barrier to progress in our field", though he (Parsons) does not agree but says that: ". . . the *role* of the observer must be explicitly analyzed and treated as part of the system". The question is how. A hierarchy of meta-observers seems to be involved; if we are to correct for the influence of one observer on a social system, we have to introduce a new observer and so on, and on each level we will get the same interaction. To the present author, D_5 seems to be *the fundamental difference*, and the one which implies the most serious methodological difficulty. However, to conclude that this entails a fundamental difference in *kind* between the opposite extremes in the difference-space is an exaggeration; not of the importance of D_5 , but of the *degree* of the interaction involved in many social science experiments. D_5 does not actually affect the *intersubjectivity* of the results, but rather their *validity*, as other observers, with the same degree of interaction may arrive at the same (but perhaps uninteresting) results.

If we do not agree that the term "science" may be applied to all points in the difference-space even with some reservations for the D_5 -

* Parsons, Bales, Shils: Working Papers in the Theory of Action P. 96.

axis, there is another, and historically important, possibility open to increase the scientific prestige of the social sciences, viz., to move them into the safe corner where classical mechanics is found. That is — one may concentrate on those social phenomena which are observable, analyzable in much the same way as the phenomena classical physics deals with. "Social physics", the term coined by Dodd, expresses very well the aim of this effort. If this is an effort to increase reliability and validity of experimental procedures and to clear the way for more refined theory formation, it is of course highly valuable. If it pretends to cover the whole range of social phenomena and to deem more soft social science trends as unscientific it may be met with the argument that it has not yet proved capable of dealing with real *social* phenomena, and to some extent is an imitation of physics dealing with less interesting data. It seems that the social sciences must go on finding their own methods and frames of reference, and that the unified science movements must try to find or define a common *minimum* for a procedure to be called scientific, rather than a common maximum which may be a barrier to progress in various fields.

THE FUNCTIONS OF MORAL PHILOSOPHY

*A plea for an integration of philosophical analysis and
empirical research.*

by

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Moral philosophy is a strange field. We may work in it for years without having any very definite opinions about its nature. One of the results is a lot of philosophizing, not about morals, but about how to philosophize about it. Luckily we do this only now and then — otherwise the subject-matter itself would cease to exist. It might be bad if we never did it, for this use of our ingenuity may show us that we are wasting it.

In this paper I want to discuss the kinds of problem with which moral philosophy, according to my view, should be concerned. No conclusive research programme will be presented. I simply want to advance a number of suggestions that may be used as starting-points for further clarification.

The classical approach.

Among the ancient Greeks the philosophy of morals did not exist as a specific discipline. Socrates was not only a philosopher of morals, he was a psychologist too, and also a sociologist and a political scientist. He had fairly definite opinions about our needs as well as about what would make us happy. He made assertions about the best type of government and about the relations between the individual and the

* The author is indebted to Dr. Arild Haaland and Dr. Knut Erik Tranøy for a number of suggestions on language and style.

state. And so did Plato, Aristotle and the Stoics. Their philosophy was problem-oriented much more than concerned with particular disciplines. They wanted to know the answer to such questions as, e. g., 'How shall we live in order to realize the good life?'. The attempt to answer such questions led them into empirical as well as analytical problems. They accepted no definite limits for their speculations in these areas. Why should they? It is we who have tried to distinguish carefully between questions of analysis of language and those of an empirical nature, and split up the study of man into a number of different sciences. Very rarely the information available and relevant for the answer to any single one of the questions of these early philosophers exists today within one and the same skull.

This growth of research has led to increased stress on the requirement of testability. But in this respect modern moral philosophers are rather similar to their classical ancestors, since neither they nor we have taken this requirement very seriously. Their thinking, as ours, consists mainly in putting forward hypotheses.

What problems are left to-day?

Suppose we make a list of all the ethical questions discussed by the Greek philosophers and remove those which at present fall within the area of science. What would then be left? The answers tend to mention two groups: First, the questions that can be answered only by a pure norm or a pure value-statement,* and, second, those which are concerned with the clarification of ethical language.

Now, moral philosophy is pursued at universities, and it is assumed that scientific research can be carried out within it. Several philosophers, however, accept the view that the philosopher, qua scientist, cannot assert pure norms or value-statements, since such statements cannot be true or false. Hence, many academic philosophers accept as a conclusion that their task is the analysis of ethical language.

This programme has dominated important parts of Anglo-American and Scandinavian moral philosophy for nearly fifty years, and so it may be useful to take it up for evaluation.

Perhaps this programme is the only possible one, or, at any rate, the most fruitful. In that case our task would be to work for greater

* Cf. below p. 42.

methodological explicitness in such studies. But does it really take care of the whole of our field? It has stimulated contacts with such other branches of philosophy as logic and semantics, but the connections with psychology and the social sciences have been almost broken. This may have been inevitable, since a certain specialization is desirable. But the whole of moral philosophy can hardly be furthered by *one* set of specialists alone. With regard to the future development, it would therefore seem more advisable if the subject could be taken up by institutes or in co-ordinated research rather than by isolated scholars. The training of moral philosophers might be changed so that their education would qualify them for taking part in team-work with logicians, semanticists, psychologists and social scientists. This would seem to provide the best possible basis for a wide and varied approach in these difficult areas.

Principles of selection.

Different principles might be taken as the basis of our examination of this topic: What a moral philosopher ought to do. For instance, we might formulate a scientific ideal stressing the importance of testability, and then ask what kind of activities would satisfy this requirement. Or we might study the history of philosophy in order to classify the problems ethicists have been concerned with, and then see whether they took up something that we are neglecting today, but might profitably take up once more. Or we might ask what sort of studies are most fruitful from the point of view of research-strategy. Certain kinds of information may be available within neighboring fields which make it convenient to take up problems permitting us to take advantage of this research. Or, finally, we might try to discover what the non-professional philosophers expect from us, or hypothesize about what kinds of problems are particularly urgent from a practical point of view.

It is clear that no single one of these considerations can be allowed to decide the issue. We might, for instance, arrive at very testable results if we devoted our whole life to counting the number of letters in sentences used in certain choice-situations. Such an activity would, however, hardly be very interesting. Or, from another point of view, lots of people want us to tell them what to do in certain types of situations. But if we sat down and wrote a new manual dictating to people how to behave, there would be nothing in this of that argumentation which is a *conditio sine qua non* for philosophical activity. — None of the

principles is therefore sufficient. On the other hand, each of them points to considerations worth making before we commit ourselves to a definite line of research.

As a point of departure I shall choose the second principle, i. e., I shall start by giving a short survey of the different types of problems with which moral philosophers have hitherto been concerned.

THE TRADITIONAL FIELDS

A. *Descriptive, analytical and explanatory part.*

I. *Methodological questions.*

- a. Descriptive studies of the methods used by moral philosophers.
- b. Suggestions of methods which, relative to certain purposes, might be used by the philosophers.

II. *Ethical behavior.*

- a. Studies of non-verbal behavior.
- b. Studies of the language and argumentation of ethical discourse (meta-ethics).

III. *Analyses of systems.*

Structural analysis, i. e., attempts to clarify and organize the concepts and statements of a certain ethical system.

B. *Normative or evaluative part.*

I. *Dogmatic part.*

Assertion of pure norms or evaluative statements* without arguments why they should be accepted.

II. *Argumentative part.*

- a. Questions of principle.
 1. Assertion of pure general norms or evaluative statements together with arguments.

* The term 'evaluative statement' is in this paper used interchangeably with the term 'value-statement'.

2. Studies of implications: E. g., if the norm N_1 or the value-statement V_1 is accepted, then we must also accept the following statements . . .

b. Applied ethics.

1. Application of norms or value-statements to *types* of situations (or objects) or to individual instances.
2. The same type of application as in 1, but by use of empirical premisses.

On the basis of this survey I shall now suggest a number of research-areas within each of the fields and indicate some new ones which might be taken up.

Methodological questions.

We do not really know how the moral philosophers work — and it may be worth knowing. E. g., which criteria have been used for selecting the problem-areas to be investigated? How does one formulate one's problem, how does one try to solve it? Which criteria have been used for deciding that the solution has been a success? Another approach would be this: we could make an inventory of the types of statement that moral philosophers usually put forward, and then discuss their frequency with reference to such categories as 1) factual statements which could be intersubjectively tested, 2) factual statements that could not be tested in this way (e. g. introspective statements), 3) factual statements falling within an established field of science, versus such that do not, 4) ethical sentences that could be translated into factual sentences, versus such that could not (so-called *pure* norms or evaluative statements), 5) sentences *about* pure norms or evaluative statements, versus assertions *of* such statements, 6) pure norms or evaluative statements advanced with reference to certain arguments versus such that are advanced dogmatically, etc.

Thirdly, we might study the philosopher's explicit or implicit definitions, and try to find out whether he follows them in actual use. What is the relation between his concept formations and the assertions made? For instance, in what ways may his conceptions change when confronted with data which he had not considered when introducing the concepts? Or, finally, how does the moral philosopher proceed in order to verify or confirm his statements?

Detailed studies of this kind seem to be a neglected field. We may summarize them in the slogan: *studies in descriptive methodology*. (For an example of research of this kind within psychology, cf. Arne Næss, *Notes on the Foundation of Psychology as a Science*, Filosofiske Problemer, no 9, Oslo 1948).

We might also start a series of normatively oriented studies. E. g., what kinds of methods seem fruitful in order to give a certain type of information? For instance, how can we best describe the norm- or value-system accepted by a certain person or group? Or how could we best clarify the concepts and procedures to be used by the moral philosopher? This field might be summarized as: *studies in normative methodology*.

Ethical behavior.

Studies of ethical behavior include investigations of non-verbal behavior, as well as studies of ethical language and argumentation (meta-ethics). Meta-ethics will be considered in the next section. In this one I shall give examples of the first type of studies. Most of them fall within social science, especially within psychology, social-psychology and social-anthropology. Whether research of this kind is *called* 'moral philosophy' or not, seems, however, rather unimportant. The important thing is that there ought to be a close connection between such studies, and investigations which are more central to moral philosophy.

1. How should one describe and classify the various types of ethical behavior? A good many systems have been proposed for the classification of ethical *language*. Concepts for the classification of ethical *behavior* have not been elaborated to the same degree. (Attempts in this direction have recently been made, e. g. by C. Kluckhohn, VI, 9).*

2. We may try to make detailed descriptions of the ethical systems accepted by a certain person, group or culture. A great variety of criteria of acceptance as well as of methods for observing whether or not a certain norm is accepted, may be used. This is a field where moral philosophers and social scientists may work together and where the already developed survey- and interview-techniques may be useful. For the *philosophical* significance of such work, it is important to preserve all the subtleties which are compatible with the exploitation of the

* See the bibliography.

research-instruments developed within social science. (Cf. R. B. Brandt, VI, 1; J. Ladd, VII, 3; S. A. Stouffer & J. Toby, VI, 42 and R. Rommetveit, VI, 39.)

3. Case-studies, i. e. intensive studies of the norms and values accepted by a specific person. They permit the research-worker to go into details of greater philosophical interest than is possible in studies on larger samples. (A case-study, from a psychological point of view, is given by N. R. Sanford, VI, 17. Cf. also the forthcoming publication by Else Frenkel-Brunswik and Harold Lasswell on the criteria of mental health accepted by psychiatrists.)

4. Studies of the origin, development and interrelations of different types of norms and values held by individuals, groups or cultures. This type of study may be seen as part of a more general investigation of the development of attitudes. Experimental research on groups may add information on this point. Psychiatric interviews and intensive case-studies may help to reveal some of the personality-determinants. Cross-cultural and comparative research will be useful in order to specify some of the cultural and national variations. (Cf. E. Westermarck's classical study, VI, 20. For a modern approach, see C. W. Morris, VI, 37. Cf. also C. Bay, I. Gullvåg, H. Ofstad and H. Tønnessen, VI, 25. For a cross-cultural approach, see E. Jacobsen & S. Schachter, III, 4).

5. Research into the consequences of different types of ethical systems for the actors themselves, their fellow-beings and the group or culture to which they belong. (Cf. J. C. Flugel, VI, 2; E. Fromm, VI, 4; Lewin, K., a. o. XII, 2.)

6. Investigations of the degree of agreement or disagreement about ethical norms or values within specified groups. (Cf. e. g., C. W. Morris, VI, 37; M. Lawlor, VI, 33.)

7. Inquiries into the factors determining the kind, and degree of agreement (or disagreement) regarding the different types of ethical statements. (Cf. e. g., M. Sherif's study, VI, 41, which, however, uses the term 'norm' otherwise than moral philosophers tend to do. H. Guetzkow & J. Gyr, "An analysis of conflict in decision-making groups", *Human Relations*, VII, 1954, takes up a somewhat related question.)

8. Studies of the compatibility or incompatibility of norms and values, and of our tendency to give a distorted picture of the possibilities of agreement or disagreement. (Cf. C. L. Stevenson, VII, 7.)

9. Investigations of the factors determining our perception of and beliefs about our own norms and values and those of other people. (Cf. L. Festinger, VI, 29 and J. Israel VI, 32 b.)

10. A study of the methods used in the successful resolution of ethical conflicts. (Cf. C. L. Stevenson, VII, 7; K. Lewin, VI, 34. See also the journal 'Conflict Resolution', Ann Arbor, Michigan.)

11. How are ethical norms and value-statements communicated to others?

12. Investigation of the determinants of different types of benevolent or aggressive behavior or emotions. — What methods can be used for creating more comprehensive identifications? (There is an enormous literature on these subject-matters. Cf. e. g. XII in the bibliography of this paper.)

13. Some moral philosophers operate with psychological theories according to which we may act from such motives as 'doing something because we believe that it is our duty' or 'doing something because we believe that it is right'. Is this conceptualization compatible with well-confirmed theories of motivation? Does any theory work with more or less equivalent conceptions? (See, e. g., C. D. Broad 'Some of the main problems of ethics', reprinted in Feigl & Sellars (ed.), *Readings in Philosophical Analysis*, N. Y. See especially p. 560. Compare E. Hilgard, *Theories of Learning*, N. Y. 1948.

14. Studies of different types of guilt-feelings and inner conflicts and their interrelations with ethical convictions. (Cf. X, I).

15. Studies of the self and the concept of the self. For instance, studies of the role of the self in deliberation and decision-processes and in the acceptance or rejection of norms or value-statements. A discussion of this concept may take the philosopher from debates within analytical philosophy regarding the nature of indexical signs, proper names and universals to questions debated by the different schools of learning-theory. How do different types of cultural milieu influence the selection of that part of the total personality which a person comes to experience as his so-called real self?

16. Analyses of ethical deliberation and decision-processes. How can we most fruitfully describe individual or group decisions at different stages of their development? In what ways may the process be expressed verbally? To what degree is the assumption about a value-calculus compatible with psychological theories about people's actual

behavior in choice-situations? How does the chooser's beliefs about the situation, his expectations, drives, wishes, norms, and values integrate at different stages of the decision-process? How is the process influenced by his perception of the environment in which he acts? In what way is the final decision distinguished from the earlier stages? (Cf. VIII).

17. Empirical studies of certain questions of validity. For instance, investigations of the relation between the acceptance of certain ethical systems and mental health or mental rigidity in general. (Cf. VI, 16, 22, 30, and 38).

The language and argumentation of ethical discourse (meta-ethics).

Since this type of study predominates in present-day moral philosophy I shall start with some critical remarks. They are intended as trend-statements which would certainly admit of a number of exceptions.

1. The collection of material is not systematic enough, and we are seldom informed about the criteria of its selection. Let us take an example. Suppose that the cognitive and emotive functions of a sentence like "The action A is right" vary along the following continuum: By some persons in some situations such sentences are used with a maximum of cognitive and a minimum of emotive functions, by other persons in other situations, they are used to fulfill strong emotive functions, and the cognitive content is nearly negligible. It follows, then, that whether the analyst will favor a so-called cognitive or emotive theory, will depend, *inter alia*, on the direction of his attention — the selection of his material. Either theory, in its extreme form, may perhaps be false for most occurrences of such sentences. But weaker forms of the two theories may both be correct when they are limited to certain fields of application. Hence, the criteria of selection come to be of crucial importance for the testing of the theories. (Cf. E. Brunswik, III, 1).

2. Most studies are not explicit enough from a methodological point of view. The author tends to express himself in such a way that it is very difficult to understand what sort of statement he is making, and what kind of evidence would be relevant. Often one is in doubt whether what is intended is an empirical hypothesis or a normative (stipulative) or a descriptive definition. Where normative definitions are proposed, we shall usually search in vain for an indication of the purposes they are intended to serve. If the definition is describing an already existing usage it is seldom indicated exactly what sort of de-

scriptive definition it is, and we rarely get to know *whose* usage the analyst is trying to describe. Finally, pure norms and evaluative statements are often intertwined with the above sort of statements. For instance, it may be asserted that, "An analysis of 'ought' shows that 'ought' implies 'can', whereas a closer scrutiny shows that what is meant is that 'ought' *ought* to presuppose can. (Cf. e. g., H. Ofstad 'Broad on Ought and Can', *Theoria*, 1955, p. 105—116, and 'Frankena on Ought and Can', to be published in *Mind*.)

G. E. Moore deserves credit for his attempts to be explicit in these respects, but even he is not always clear. At several places in his *Ethics* it seems rather difficult to find out whether his statements are meant as theoretical assertions by a meta-ethicist or as pure norms proposed by a moralist. (Cf. e. g., his discussion in Ch. V 'Results the Test of Right and Wrong'.)

3. Many studies tend to concentrate on isolated words and expressions instead of analysing them as parts of an ethical system. E. g., Moritz Schlick says in *Problems of Ethics*, (trans. by David Rynin) that he wants to analyse what we mean by 'responsibility'. And then he answers that the person P is responsible for a certain damage if P is the person on whom punishment will work as a means of preventing repetitions. Such an analysis is useless when it is presented as an analysis of the concept of responsibility. More may be said in its favor if it is presented as an analysis of this concept as part of certain utilitarian or teleological views on punishment.

Or take some of the countless analyses of 'good', 'right', 'ought', 'freedom', etc. Very often there is the same tendency to talk about the term as an isolated entity instead of analysing it as part of an ethical context. Examples of the resulting failures may be found in papers which on the basis of an analysis of the concepts 'free' or 'ought' try to find out what conditions must be fulfilled if a man is to be responsible. This procedure may lead astray, because important requirements may be implied by the *system* without being implied by these *concepts*. If a judge says to a man 'You ought to go to jail for six years', his judgment may *presuppose* that the man is not insane, without logically implying this assertion. Some of the most important requirements of responsibility can be found only through an analysis of what ethical or legal judgments presuppose, and only by analysing the judgments as

parts of ethical or legal systems. By focussing on the term 'ought' and its possible entailments, the analysis becomes too narrow.

4. It is semantically unfruitful to presuppose that there exists something like *the* ordinary usage of ethical terms. Maybe a certain interpretation has a high frequency. This, however, should not be assumed at the outset, but be proposed as a hypothesis, or come as the result of research.

5. The analysts trying to make clear the relation between the analysandum and the analysans have often concentrated too much on finding what they call 'synonyms' or 'equivalent expressions'. They tend to work with very strict requirements regarding the relation between analysandum and analysans. To search for synonymous or equivalent expressions is only *one* of the tasks and one which severely limits the freedom of the analyst. It may often be more fruitful to search for explications, precisations, and specifications, etc., concepts which seem well-adapted in dealing with natural languages. (Cf. Arne Næss, III, 9, and H. O., VIII, 8).

6. The general orientation in meta-ethical studies is towards the advancement of hypotheses. Attempts to work out methods for testing the hypotheses are neglected. The emphasis is on good ideas, rather than on testable results.

7. The attack on a problem is often determined by the instruments so that the philosopher feels through with his work when his instruments become useless. This has the consequence that problems which constitute a whole are split apart. Furthermore, the philosopher tends to formulate his problems in such a way that he protects himself against their empirical implications. His lack of familiarity with empirical problems makes him unable or unwilling to perceive them so that their empirical implications become explicit.

Publications within meta-ethics consist, *inter alia*, of more or less interesting hypotheses about the use of different ethical expressions and concepts. "What is the meaning of the concept 'good'?", G. E. Moore asks, and answers that it is indefinable. For a while this idea is accepted by a certain group. Later on, others suggest that 'good' partly has an emotive meaning. This hypothesis seems to solve certain difficulties which the previous one left unsolved. Time is ripe for a change, and the emotive theory becomes *à la mode*.

Modern moral philosophy is speculative in important respects, as

was the ancient philosophy which tried to formulate ideals and norms for human conduct. But modern philosophy, instead of speculating about what people ought to do, speculates about what it *means* to say that people ought to do something. Since books where it is written what we ought to do are irritating to some, and books where it is written what it *means* to say that we ought to do something, are irritating to others, one difference between old and modern moral philosophy is that today we irritate other people than before.

8. Meta-ethics, and what we might call 'ethical ontology', are sometimes confused. Some authors argue as if semantical analysis of norms and value-statements could show that the intuitionists are wrong when they assert that there are objects with so-called 'indefinable, non-natural properties'. But this ontological assertion is independent of the results of semantical analysis of such statements. Even if the intuitionist should be able to show that our pure norms and value-statements assert the existence of such a property, that is no conclusive evidence that it does exist. And even if the empiricist should be able to show that these statements express prescriptions or imperatives, that is no conclusive evidence that it does not exist. No deductive inference can be made from premisses stating something about verbal phenomena to a conclusion asserting the existence or non-existence of a certain property of the non-verbal part of reality.

I turn to a more constructive task: to indicate certain problem-areas within meta-ethics which may be interesting and fruitful.

1. *Descriptive studies of ethical language.* Within meta-ethical writings the object-language makes use of such terms as 'good', 'bad', 'ought', 'right', and 'duty'. How representative is this selection of the language used in actual choice-situations or in other types of ethically relevant situations? It is my conjecture that the ethical vocabulary actually used is much richer than this sample, and permits of distinctions which seldom are discussed by the analyst. People reason not only in terms of 'good' and 'right', etc. They also use such forms as 'I recommend that . . .', 'I advise that . . .', 'Why not try to do something like this . . .', 'Perhaps it might be a good thing to do . . .', 'It is hard to say, but if I were you I probably would have acted this way . . .'. The pressure on the receiver may vary with the

expression used, as may also the attraction attributed to a certain object. Such studies of the ethical vocabulary may be supplemented with investigations of its determinants, thereby connecting meta-ethics with studies of non-verbal behavior. (Cf. the works mentioned in "Empirical studies of language which may have methodological significance for meta-ethics" (VII)).

2. *Empirical studies of the use of ethical terms relative to the system within which they are used.* E. g., how does a person who accepts an ethical system of the following kind . . . use the expressions 'value', 'good', 'bad', 'right', etc.?

3. *Empirical studies of argumentational patterns.* How do people actually reason and argue in different types of ethical discourse? Several hypotheses have been proposed, but we have no systematic knowledge. What sort of statements will, under which conditions, be considered as relevant — or even as conclusive — arguments for or against a certain ethical statement? If by 'the meaning' of an ethical statement, we refer to its truth-conditions, studies of this kind may give us information about its meaning. Under what conditions will the arguments consist of factual statements? Under what conditions will they consist of sentences using ethical terminology? What are the criteria of valid argument applied? At what stages in the argumentational series will there be a shift in the type of argument used? At what stages do different persons consider continuation of argumentation useless?

4. It is my conjecture that the best approach at the present stage is a combination of 2 and 3, i. e., *studies of units of ethical argumentation.* By 'units of argumentation' I refer to a pattern of statements: factual, normative and/or evaluative, in so far as they function as parts of the argumentation. We must try to study these matters in living contexts, and in representative situations. Through this procedure we may hope to obtain some knowledge about the applied criteria of valid and invalid argumentation, about instances of argumentation which are considered to be valid (respectively: invalid) and about the psychological and social-psychological functions of the language used. We may try to establish statements of the two following kinds: (1) 'If a person with the following characteristics . . . and who accepts an ethical system of this kind . . . in such and such situations . . . argues about a topic of the following nature . . . and makes the following statements . . .

then he may consider the following . . . as a criterion of (definition of) a confirming (conclusive) argument for a certain pure norm or evaluative statement'. (2) 'If a person in such and such situations, where he fulfills this or that role and has the following perceptions, needs, and beliefs concerning a certain matter, and where he argues about this matter and makes an ethical sentence such as the following one . . . which has the following place in the argumentational pattern . . ., *then* he will accept the following set of sentences . . . and reject the following set . . . as substitutes of the sentence advanced, and the advancement of the sentence will tend to fulfill the following functions . . .' The variables included in these sentential schemas are, of course, included mainly for the purpose of illustration.

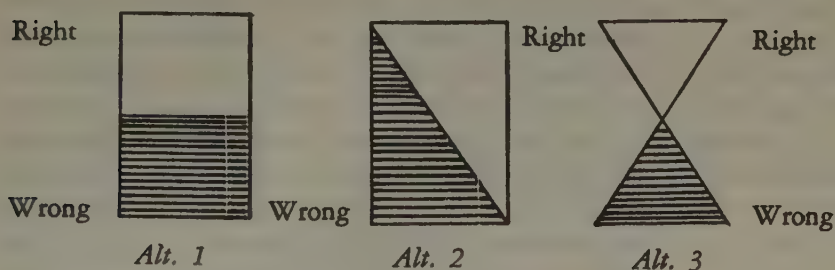
5. The studies referred to in 1, 2, 3, and 4 include descriptive and explanatory studies of an empirical kind. Within groups 2, 3, and 4 studies which are *explicative* in nature are certainly also called for. In addition to finding out the actual use of certain expressions in certain contexts, we must try to analyse these expressions, explicate them, try to make them more precise. Explicative studies, or studies aiming at some sort of reconstruction, might be taken up also with units of ethical argumentation as their object. Research of this kind is flourishing to-day. (Cf. VII).

6. To propose different criteria of valid argumentation and judge and evaluate instances of ethical reasoning in view of them.

7. So far I have not mentioned studies trying to work out the *logical* structure of ethical concepts and systems. The development of modern logic provides us with a powerful tool for such analyses. (Cf. 'Meta-ethics and formal logic' in the bibliography). We must, however, guard ourselves against the misconception that the research-situation in meta-ethics would be dramatically improved if only the essays were written by use of symbolic logic rather than by use of natural languages. The important thing is to obtain greater clarity and precision. The mere symbolic *form* of a sentence is no guarantee of this.

8. A fascinating area is the advancement of *possible* conceptual systems for normative or evaluative ethics or for meta-ethics. Suppose we create an ethical system with *recommendation* as basic concept leaving out the traditional concepts of right, duty, good, bad, etc. What would be the implications of a system of this kind? What functions could it fulfill? To take another example: What are the implications

of constructing the relation between 'good' and 'bad' and between 'right' and 'wrong' as a dichotomy? What will be the implications of constructing it as a continuum?



According to alternative 1, that which is right or wrong, say, an action, is *either* the one *or* the other. According to alternative 2, an action can be more or less right or more or less wrong, and the categories overlap so that the action can have both characteristics, except when it obtains a maximum of one of them. According to alternative 3, an action can be more or less right or more or less wrong, but cannot have both characteristics. And this, of course, is only one very simple set of alternatives. Possible worlds are waiting to be explored.

9. The traditional expressions for naming or describing different values, goal-objects or ends in view, tend to be rather imprecise. We use such words as 'happiness', 'pleasure', 'virtue', 'to do what is right', 'love', 'knowledge', 'perfection', etc., without connecting them with methods of observation. This impreciseness is probably one of the reasons why moral philosophy is not consulted more than it is. In order for the value-description to be useful for decision-makers it must be made concrete and connected with operational indexes. Such work can be done only in close contact with the social sciences. Then it may be possible to describe the values or goal-objects in such a way that observers can report on whether or not, or to what degree, a certain trend or state of affairs in society corresponds with the value-description. Studies of this kind, we may call '*studies in the empirical basis of the value-description*'. (Cf. H. Lasswell & McDougall, X, 3).

Analyses of ethical systems.

Descriptions of ethical systems accepted by a certain person or group, I classify as studies of ethical behavior. By 'analysis of systems'

I refer to the analysis of such ethical systems which either are parts of the ethical tradition, e. g., utilitarianism, or which are constructed as objects of analysis.

1. Studies may be made which analyse and criticize the concepts and statements of a certain ethical system and try to organize them so that their logical structure becomes explicit. The term 'structural analysis' may be used as a name for this.

2. We may try to construct the ethical system which is compatible with a certain set of ethical utterances. For instance, we may try to describe or construct the ethical system which is implicit in the writings of Gandhi or Albert Schweitzer, ordering the statements into higher and lower norms. Probably we shall find that the utterances of a certain author are compatible with several different norm- or value-systems, and it may be interesting to point out these different possibilities. For instance, we may arrive at one set of interpretations if we construct the system on the basis of the author's *examples* of right and wrong and at another if we try to derive the norms which are implicit in his more *general* statements. (Cf. J. Galtung & A. Næss, IX, 4).

Normative or evaluative part.

I distinguished between *dogmatic* and *argumentative* normative or evaluative ethics. 'Dogmatic normative or evaluative ethics' refers to the assertion of pure normative or evaluative statements without giving arguments for them. This activity consists of pure speculations and should be excluded from philosophy, not because it is speculative but because it is an uninteresting form of speculation. (Modern meta-ethics, in criticizing traditional moral philosophy, has sometimes misrepresented this tradition by making it more speculative and less argumentative than it actually is.)

Within moral philosophy I should, however, include the assertion of pure normative or evaluative statements in so far as the philosopher gives arguments for them. Such statements may fall within the principal part, or be instances of applied ethics. The important point is that the steps of the argumentation are clearly shown so that the reader can check the premisses and the way in which the author has tried to arrive at his conclusion. (See, e. g., the studies mentioned in IX).

By neglecting this branch of study we have failed to use an important possibility for coming in contact with the problems which con-

cern us as human beings, and to try to improve the reasoning in such cases.

The dogmatic as well as the argumentative part of normative or evaluative ethics belong to the division which we could call 'the norm-or value-asserting division'. In addition to this we may also include another part which we could call 'the norm- or value-presenting division', which *presents* norms and value-statements without *asserting* them. So whereas the divisions within part A in the survey of the traditional fields of moral philosophy (cf. p. 38) seem useful, part B might rather be divided in this way:

B. *Normative or evaluative part.*

α. *Norm- or value-asserting division.*

I. Dogmatic part (which I do not consider as belonging to philosophy).

II. Argumentative part.

a. Questions of principle.

b. Applied ethics.

β. *Norm- or value-presenting division.*

I. Possible ethical systems.

II. Applied ethics (without ethical commitments.)

I proceed now to an indication of the type of research which may be included within the β-division.

1. A promising, but neglected field, is the development of *possible* ethical systems. Most of us accept quite uncritically a certain variation of the ethical system of the group to which we belong. What are some of the alternative systems that we might accept? We should permit ourselves to react towards a much richer selection of ethical systems than we usually do. Within this research-area we can give our desires and fantasy free flow and construct without asserting. The approach is: 'Look at this ethical system . . . What do you think about that? What arguments do you have for and against it?' The attitude of the constructor is not: 'Obey these rules!'

2. Just as the legislator and the legal theorist influence the behavior of the judge, and just as *his* verdicts, on the other hand, influence both,

so normative and evaluative ethics as well as meta-ethics, need the confrontation with social reality which comes through applied ethics. Within this field strategy-research seems to me especially important.

By 'strategy-research' I mean an investigation which, taking as its starting-point a certain personal or social choice-situation, tries to clarify a certain set of normative or evaluative premisses and to obtain evidence regarding the probable consequences of a certain set of alternative courses of actions or series of actions in relation to these norms or values. The following steps must be taken: the choice-situation must be described, the range of possible alternative action-series must be indicated. If we have a choice between different actors at different stages of the development of the strategy, their relevant characteristics must be described. A set of norms or values must be given and connected with methods of observation. The ethical system in question (the norms or values) may be the one accepted by the actors, by the research-worker, or it may be especially constructed for the case in question, etc. (Instead of presupposing a set of norms or values, we might presuppose a set of preferences. Cf. R. B. Braithwaite, X, 1.)

Let us try to indicate the kind of statements which may be established in this way. But first we make the following suppositions: 1) only the two action-series Aa and Ab have to be considered, 2) each of them consists of only two actions, Aa₁ and Aa₂, respectively Ab₁ and Ab₂, 3) the action Aa₁ or Ab₁, will be done by the person P₁ and the action Aa₂ or Ab₂ will be done by the person P₂, 4) no norms are involved, 5) only three values V₁ V₂ and V₃ have to be considered, 6) each of the values can be realized in different degrees, varying on a scale from 0—10.

On these suppositions the following kind of statements may be established: 'Given the values V₁ V₂ V₃ and the two action-series Aa and Ab, then the following is true: the action Aa₁ by the person P₁ will probably realize the value V₁ to degree 3, value V₂ to the degree 5 and the value V₃ to the degree 3. The next action in this action-series, Aa₂, performed by the person P₂, will probably realize the value V₁ to the degree 4, value V₂ to the degree 5 and the value V₃ to the degree 4. The alternative action-series, Ab, will probably have these consequences: Ab₁, performed by the person P₁, will realize the value V₁ to zero degree, value V₂ to the degree 10 and the value V₃ to the degree 1. The next action, Ab₂, performed by the person P₂, will realize the value V₁ to the degree 1, the value V₂ to degree 10 and the value

V_3 to the degree 4.' Further complications arise if the policy-maker has to take into account the possible effects of the counter-strategies selected by persons affected by the actions. Moreover, from the above kind of statements we cannot arrive at a conclusion about which action-series is to be preferred, unless we introduce a new set of value-premisses according to which we can compare varying degrees of the different values.

Research of this kind seems to me fruitful for several reasons, of which three may be especially mentioned: 1) information of this kind is of personal and social importance because it helps to base decisions on available information, 2) it may help to decrease the impreciseness of much of the traditional ethical terminology, 3) students wanting to take up moral philosophy are often interested in normative, not only meta-ethical questions. By channelling their interests towards strategy-research it may be possible to preserve this motivation and do scientific work at the same time. (Cf. X).

3. Strategy-research is related to such ethical systems which try to find the right course of action by paying attention to the consequences of the different alternatives. We may, however, make studies of this kind also for other types of ethical systems. In general, given a system with decisive emphasis on the conditions $x y z$, we may investigate which action, in a certain situation or type of situation, will fulfill these conditions.

4. No matter what ethical system a person accepts, there seems to be at least one norm which he — by maintaining that a certain action ought to be done — must accept. Suppose the person P in the situation S is confronted with a choice between A_1 and A_2 , and that this ethical system, paying attention only to certain characteristics of the actions themselves, has the implication that A_1 ought to be done. Nevertheless, he must also accept a norm saying that the fact that A_1 has the required characteristics, whereas A_2 does not, is more important from the point of view of what ought to be done, than any other difference that might exist between them. For instance, it is more important than the fact that A_2 's consequences are more beneficial than A_1 's or that A_2 will be done from a good motive whereas A_1 will be done from a bad motive. The actor must admit that these things ought to count less than the difference between the actions themselves. (By saying that the actor 'must accept' this norm, I mean that his rejection of it will be in conflict with his own norm saying that A_1 ought to be done.)

In order to make the point more general, we may introduce the concept of a *difference-series*. By 'the difference-series between the courses of actions A_1 and A_2 ', I refer to all the factors which will be different in case we choose the one rather than the other alternative. Perhaps the motive of the actor will be different, the actions themselves will be, and perhaps the consequences, too, will be different — all these factors, then, are parts of the difference-series between A_1 and A_2 . Ethical systems usually decide whether A_1 or A_2 ought to be done by paying attention to *some* of the factors within the difference-series, other factors are considered to be ethically irrelevant. Let us suppose that a certain ethical system decides whether A_1 or A_2 ought to be selected by investigating whether A_1 or A_2 realizes the factor x within the difference-series to the highest degree. Let us suppose that A_1 realizes this factor to the degree 3, whereas A_2 realizes x only to degree 1, and that A_1 , therefore, ought to be done. Let us further suppose that the difference-series includes only two other factors, y and z , and that A_2 realizes these two to the degree 4, whereas A_1 realizes them to degree 1. Then the actor who maintains that A_1 ought to be selected, must also accept the following norm: 'The fact that A_1 realizes the factor x to the degree 3 whereas A_2 realizes this factor only to the degree 1 is — from the point of view of selecting the action which ought to be done — ethically more significant than the fact that A_2 realizes the y - and z -factor to the degree 4, whereas A_1 realizes them only to the degree 1'.

This norm may be called 'the action-implied norm', since it is implied by what we are doing, regardless of the type of ethical system that we accept. The norm says, loosely speaking, that we ought to ignore what we in fact ignore, and this norm, it seems to me, must be accepted, in so far as we assert that we ought to do what we in fact do. (The precise contents of the action-implied norm may vary with whether the actor asserts that A_1 objectively, subjectively or putatively ought to be done. We do not have to go into these modifications here.)

Research trying to formulate the norm which is involved in a certain choice, makes us more aware of the ethical implications of what we do, or intend to do.

SURVEY OF SUGGESTED RESEARCH-AREAS

A. *Descriptive, analytical, and explanatory studies.*

I. *Methodological questions.*

1. Studies in descriptive methodology.
2. Studies in normative methodology.

II. *Ethical behavior.*

a. *Studies with emphasis on non-verbal behavior.*

1. Development of conceptual systems in terms of which ethical behavior may be described.
2. Descriptions of ethical systems accepted by a certain person, group, or culture.
3. Case-studies: intensive studies of norms and values accepted by a specific person.
4. Studies of the origin and development of different types of ethical systems. Developmental studies, cross-cultural and comparative research, group-experiments, and intensive case-studies.
5. Studies of the psychological and sociological consequences of different types of ethical systems.
6. Investigations of the degree of agreement or disagreement regarding certain norms and values.
7. Studies of the factors determining the degree of agreements regarding certain norms and values.
8. The compatibility of different ethical norms and values. Our tendency to give a distorted picture of the compatibility.
9. Studies of the factors determining our perceptions and beliefs about our own ethical systems and those of others.
10. Studies of methods for resolving ethical conflicts.
11. The communication of ethical norms and values.
12. Determinants of different types of ethically relevant behavior, e. g., benevolent or aggressive behavior.

13. Studies in ethical motivation.
14. Studies of guilt-feelings and inner conflicts and their relation to ethical systems.
15. Studies of the self.
16. Analyses of ethical deliberation and decision-processes. Individual and group-decisions.
17. Studies of the validity of ethical norms, e. g., the relation between the acceptance of certain norms and mental health and mental rigidity.

b. *Meta-ethics.*

1. Descriptive studies of ethical language.
2. Empirical studies of the use and function of certain ethical terms.
- 3—4. Empirical studies of units of ethical argumentation. The actually applied criteria of valid reasoning and the different functions of the language used.
5. Explicative studies.
6. Studies of the validity of ethical norms, e. g., the proposal of different concepts of valid argumentation and evaluation of instances of ethical reasoning in view of them.
7. Study of the logical structure of ethical concepts and systems.
8. Proposals of possible conceptual systems for normative or evaluative ethics and for meta-ethics.
9. Studies of the empirical basis of the value-description.

III. *Analyses of ethical systems.*

1. Structural analysis.
2. Construction of ethical systems which are compatible with a certain set of ethical statements.

B. *Normative or evaluative part.*

a. *Norm- or value-asserting division.*

- II a & b. Argumentation in favor of certain pure norms or value-statements.

β. *Norm or value-presenting division.*

- I. The advancement of possible ethical systems.
- II.
 1. Strategy research.
 2. Attempts to describe the action which, among certain alternatives, fulfills the requirements of a certain ethical system.
 3. Studies of the action-implied norm.

Two schematic representations.



Fig. 2.

Schematic representation of the interrelations between moral philosophy and some of the social sciences. Each science is delimited by a set of dotted lines. The heaviest line marks off the area which is unique to the discipline in question. The section lying between the heavy and the lighter line consists of the problems which, although belonging to the discipline in question, come closer to problems dealt with also in other sciences. Such problems do not arise only between sciences which here are made to lie side by side. All lines are dotted in order to show that interaction can go on between any areas. Moral philosophy has, somewhat arbitrarily, been marked off by three lines, meta-ethics being the section which is most central to moral philosophy.

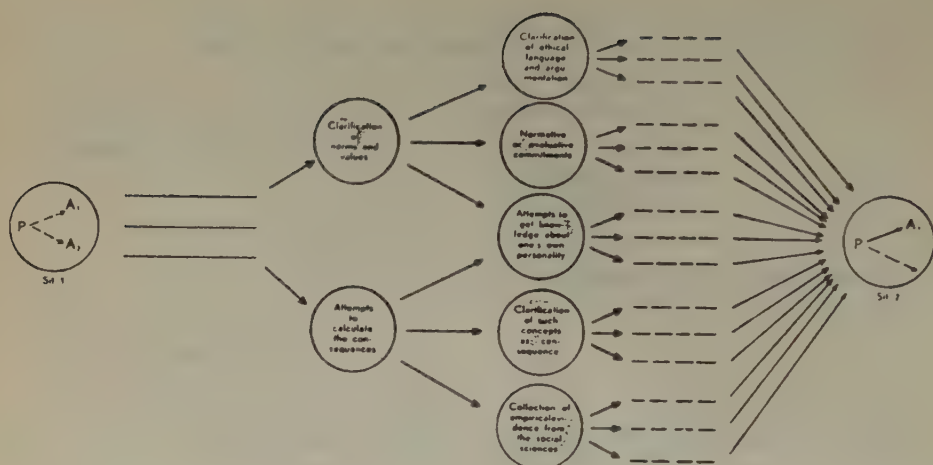


Fig. 3.

Schematic representation of the interrelations between the problems of moral philosophy, showing how they are connected for a chooser. Each problem gives birth to new ones. In Sit. 1, his deliberation starts. In Sit. 2, he has decided that A_1 is the right action.

Research-priorities.

We have not taken a final position on the tasks of moral philosophy until we have said something about the priority of the different research-areas indicated.

To rank these areas is in fact to give advice, e. g., to a student wanting to do research in moral philosophy or to institutions wanting to support research. Such advice, however, will obviously vary with the case in question. It will depend, e. g., upon the special abilities of the student, his interests, the achievements of the milieu or institution within which he will be working, etc. But let us assume that none of these factors favors any special area. Then the only important consideration is the development of moral philosophy. I should then divide the research-areas into two groups, one consisting of those I believe to be especially important, the other of those I consider less important or less urgent. Any sort of refined ranking seems out of the question.

The first group includes the following fields:

1. Studies of ethical language and argumentation.
 - a. Empirical studies of the criteria of valid ethical reasoning which actually are applied by a certain person or group in ethical deliberation.
 - b. Empirical studies of the use and philosophical and psychological functions of ethical language (and arguments) in different types of ethically relevant situations.
 - c. Explicative studies (e. g., studies aiming at logical reconstruction) of the use of ethical language.
 - d. Explicative studies of types of ethical reasoning.
 - e. Proposals of possible criteria of valid ethical reasoning (valid ethical norms and value-judgements), and investigations of whether certain norms or value-statements are valid according to any of these criteria.
2. Proposals and constructions of possible ethical systems.
3. Strategy-research and studies of the empirical basis of value-descriptions.

The expression 'empirical studies' (cf. 1 a & b), is meant to cover also the formation of hypotheses and the construction of theories. The explanatory part of moral philosophy must be constructed as hypothetico-deductive systems. So far it has neither taken sufficient advantage of the development of theory-construction and application of formal systems to the study of behavior, nor of the refined methods of observation of behavior and of construction of experiments, developed within the social sciences.

Other things being equal, priority must be given to *basic research*, i. e. research dealing with problems which are fundamental to the whole discipline. In moral philosophy basic research must, loosely speaking, be concerned with the nature of ethical norms and value-statements and their validity. Problem areas 2 and 3 are not concerned with research of this kind. I rank them equal with the other ones for four reasons: 1) they are of human importance, 2) they are neglected in modern moral philosophy, 3) they represent fields which may benefit from the development of social sciences, 4) studies in these fields may have important repercussions for basic research.

Within the explicative studies (1c and d), it seems to me that studies dealing with natural languages should be given priority over studies of artificial ones. The latter type of study, in so far as it can contribute to basic knowledge, presents us with a problem of application which can be met only if we have knowledge about the use and functions of natural languages.

Two examples of the planning of studies integrating semantical analysis and empirical research.

Numerous examples of studies integrating semantical analysis and empirical research might be mentioned. Here I want only to indicate the planning of two projects within basic research. The first deals with the nature of ethical norms, the second with some aspects of their validity.

Example 1. Suppose we are interested in a problem which we roughly may describe as 'the meaning of ethical norms'. This phrase may, of course, refer to an indefinite number of different problems. Suppose we choose to reformulate it like this: 'What determines the degree to which ought-sentences are used emotively rather than cognitively?' (We assume, in other words, that they may be used in both ways.) In order to proceed with our problem, concepts like 'use', 'ought-sentence', 'emotive', 'cognitive', would have to be made more precise. By 'ought-sentence' we may, for instance, mean any sentence in accordance with the skeleton form, 'x ought y'. But still our question cannot be answered. And we split it up in three new ones, one regarding which other sentences might be substituted for the ought-sentence, another concerning its possible truth-conditions and a third problem regarding some of the psychological or social-psychological correlates of its use.

In order to attack the first problem, we may (on the basis of previous experience, assumptions, and more or less explicit hypotheses), make a list of sentences which we hope will give a sample of imperative, normative and descriptive sentences. We include sentences coming as close as possible to pure emotional outbursts, we include imperatives, etc., and finally declarative sentences making use of no ethical terms whatsoever. Of each of these types of sentences we include a number sufficiently large to permit the test-person to select all his substitutions

from one of the types alone. Now, the grammatical form of a sentence is no reliable symptom of its emotive or cognitive use. This reservation must be kept in mind when investigating which sentences on the list the assessor of the ought-sentence is willing to accept as possible substitutes.

As a next step we may create a group discussion of an ethically relevant topic, e. g., the justification of euthanasia. The discussion is recorded, and when it is over, we play the record for the participants (for each of them separately) until we reach his first ought-sentence. After he has heard it, the playing is interrupted, and the person is presented with our list of possible substitutes. For each sentence he is asked whether he might have used that sentence just as well as the one he actually did use. (The instruction will vary depending upon the information we want to get). In case he answers affirmatively, the discussion is recorded over again with the new sentence instead of the old one — but within the old context. This record may be played to the person in order that he may decide whether he accepts the substitution. When we have gone through the list, his answers may perhaps be expressed by the use of an index of normative versus descriptive substitutions, the number being higher the more the former prevail as compared with the latter. The same procedure is then repeated for the other ought-sentences.

Hypotheses may be developed about the determinants of the value of this index for a certain person's use of a certain ought-sentence in a certain situation. We may find that it varies, *inter alia*, with such factors as the place of the ought-sentence in the argumentational pattern, the degree to which the speaker is emotionally involved in the topic discussed, his ability or willingness to specify the truth-conditions of the ought-sentence, the degree to which he wants to commit himself, take a standpoint, influence others, his role in relation to the other participants, etc.

One way of approaching the second question concerning the possible truth-conditions of the ought-sentence, might be to ask the person about the conditions under which he would consider it as true or confirmed as against false or disconfirmed. (We might, of course, also design the study so that we present him with a list of possible arguments or truth-conditions, from which he might choose. The fact that the person indicates some intersubjective test-conditions may be con-

sidered as confirming (to some degree) a hypothesis about cognitive use. His inability to specify such conditions cannot, however, be regarded as proving that the sentence is used emotively. Psychological and physiological methods may be applied for studying whether ought-sentences fulfill emotive functions. What we shall find, I believe, is that the more the ought-sentences are used within affectively toned areas of discussion, the more they will — other factors being as equal as possible — be used emotively. But this finding will hold, I guess, also for is-sentences. Hence, the belief that ought-sentences express emotions whereas is-sentences do not, may partly be due to the fact that a higher percentage of ought-sentences is used within affectively toned areas.

The psychologist can never do better than to establish correlations between emotional states and the production of certain sentences. He is unable to creep inside the sentence and show every one that there was an emotion. He can, therefore, never disprove the intuitionist if *he* claims that he has been inside and has seen that there was no emotion. However, the fact that a person can specify no intersubjective test-conditions of a sentence he makes, together with the fact that it correlates with measures of emotional states, give some confirmation of the hypothesis that it is used emotively.

(The two other tests: the substitution-test and the truth-condition-test, must also be carried out for is-sentences. Otherwise we shall not know whether the traits we find are typical of ought- as well as of is-sentences or whether they characterize the former type only. Perhaps ought-sentences sometimes are mentioned as substitutes for an is-sentence just as frequently as is-sentences are mentioned as substitutes for an ought-sentence.)

Example 2. Suppose we want to study the problem of the validity of ethical norms.* The expression 'the norm N is valid' is used in different ways, and we may distinguish between the following five precisations:

1. The norm N is generally accepted.
2. The norm N exists somewhere as a platonic idea.
3. The norm N is true.

*) Cf. H. O.: 'Objectivity of Norms and Value-judgments according to recent Scandinavian Philosophy', *Philos. & Phenom. Research*, Vol. XII, 1951.

4. Arguments can be given in favor of N.
5. The norm N is in accordance with human nature.

None of these five formulations refers to one specific problem. They are starting-points for further analyses. Investigations of several of the problems indicated, first of all of 1. and 5. will take us into empirical research. Suppose we select 5. Its formulation may be made more precise, for instance, by use of this formulation: 'Are there norms which are in accordance with human nature in the sense that no matter what punishment we are subjected to for following the norm, and no matter what reward we would have been given for disregarding the norm, we would prefer a life devoted to the norm rather than one disregarding it?' The best way of finding out which life we would prefer would be to live several lives, one according to the norm, another in disregard of it — but otherwise as similar as possible — and then see which life we would prefer to live over again, assuming that our memory is infallible. This way is blocked. As the world is now, we have troubles in managing to live one life, let alone three. The closest we can come, it seems, would be to get data on four groups. Within the *first group* the persons follow the norm, (suppose the norm is 'We ought not to be sadistic towards anyone'); more than that, they live on very friendly terms with all their compatriots. Different types of reward and no punishment are given to them for behaving in this way. Within the *second group* the persons break the norm and different types of rewards and no punishment are given to them for behaving sadistically. Within the *third group* the people obey the norm, but are suppressed and punished for doing so, whereas in the *fourth group* the persons disobey the norm and are suppressed and punished for doing so. We may then raise, for instance, the following questions: 'Is there a sense in which the persons belonging to group 1 thrive better than persons belonging to group 3?' A question which probably will be answered in the affirmative. Next, 'Is there a sense in which the persons of group 1 thrive better than persons of group 2?' Third, 'Is there a sense in which the persons of group 3 thrive better than the persons of group 2?'

None of these questions can be directly answered, since such groups do not exist and cannot be created. The three questions refer to an idealized research-design. We may, however, be confronted with groups whose characteristics are approximations to the conditions indicated.

The third question is the crucial one. Is it possible that the gratification of following a certain ethical norm may be so deep and enduring that this gratification counts so much more than the pains and suffering involved, that a life in accordance with the norm may be preferred to a life in disregard of it? Even if this question should have to be given a negative answer, it is, of course, important to study the degree to which such relations hold *relative* to certain types of persons or environmental conditions. For instance, if group 3 consists only of persons who obtain strong gratifications for behaving in accordance with their conscience, the question above may be answered affirmatively.

Conclusion.

According to the view developed in this paper, moral philosophy is not devoted to a subject-matter sharply distinguished from other disciplines. It is characterized by the type of problems with which it is concerned, problems which emerge from the fact that we make judgements about right and wrong, good and bad. The methods for dealing with these problems may be speculative, analytical or empirical. They will vary, depending on the aspect of the problem. Some of the problems discussed in moral philosophy are specific for this discipline in the sense that they are not discussed in other disciplines and may be dealt with by students who have taken courses in philosophy. Other problems have a common ground with other sciences, especially with psychology and the social sciences. Moral philosophy should try to take advantage of the rich development within these disciplines in order to further scientific research.

It has been argued that moral philosophy is not concerned with empirical problems because ethical language cannot be translated into a so-called naturalistic language. This argument, however, confuses object-language and meta-language. Even if it is true that the object-language of meta-ethics is so-called non-naturalistic, some of our statements *about* sentences in this language may be empirical. Furthermore, meta-ethics is not the whole of moral philosophy. Strategy-research, for instance, has clearly empirical components.

Considering our own research over years, I believe that a life where different activities, theory-construction, analysis and empirical investigations interchange with each other, will be preferable. Why should we

not favor the good life of research in Plato's sense, consisting of a harmonious relation between different activities? And what is our alternative? Without contact with other sciences some of the most important problems of moral philosophy will be encapsulated and the discipline itself may degenerate into intelligent conversation or the cultivation of a certain technique which will be used only by those who want to learn this technique.

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* Cf. VII, 3.

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DIALOGUE ON THE HYPOTHETICAL CHARACTER OF LOGICAL ANALYSIS.

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It is not unrealistic (nor would it matter much if it were) to imagine the two sentences "Norway is a democracy" and "Norway is not a democracy" to be printed within the same text (with some space between them).

If the phenomenon is noticed by a logical analyst his reaction may well be such as to make this an adequate rational reconstruction of it: the two sentences are pictured on the same line with an "and" between them; the conjunction is symbolized as " $p \cdot \bar{p}$ "; the analyst remembers that " $p \cdot \bar{p}$ " is the simplest contradiction in the sentential calculus (in the usual formulation of it); and the analyst asserts: there is at least one contradiction in the text. (Something of the sort may actually occur if the analyst is asked for a *justification* of his verdict.)

But even this innocent little piece of logical analysis is impure with empirical presuppositions. Notice, e. g., that our analyst asserted: there is at least one contradiction *in the text*. But the formula " $p \cdot \bar{p}$ " is not printed in the text, and it was *this* formula that the analyst remembered to be a contradiction.

If a logical analyst is asked to justify his verdict "S is a contradiction" or "S is analytic" (where S is a natural language sentence), one frequent type of answer is "Because S can be turned into a formal contradiction", like " $p \cdot \bar{p}$ ", "Because S can be turned into a tautology", like " $p \vee \bar{p}$ ", " $p \supset p$ ", etc. Such an answer *indicates* a justification. However, justification by reference to formal systems is no safer than the *prima facie* more obscure references to the meanings of the terms and sentences involved, since, it will be argued, the former is relevant only if the latter is included.

We arrange a dialogue between LOG and SEM on the topic. LOG is a philosopher or scientist who employs formal systems in his examination of arguments and doctrines which are formulated in natural languages, i. e. he is a *logical analyst* in one frequent sense of that term. SEM is, e. g., the present author, who, in the year 1957, feels little resistance against being classified as a member of the Oslo group of empirical semantics. (The points made in sections 3, 4 and 6 below are typical of the group.)

1 LOG: There is at least one contradiction of the form " $p \cdot \bar{p}$ " in the text.

SEM: What do you mean by "of the form " $p \cdot \bar{p}$ ""? The symbol " $p \cdot \bar{p}$ " does not itself occur in the text, and there is no substitution instance of it either.

LOG: I know. But let the first " p " stand for a proposition, and the second " p " for the same proposition. Also, let " \bar{p} " with a bar over it (" \bar{p} ") stand for the negation of p : i. e. the proposition that *must* be false if p is true, and *must* be true if p is false. Finally, let the dot (" \cdot ") stand for conjunction. A conjunction is a combination of two or more propositions such that if each constituent proposition is true, then the combination of them must be true, and if one or more constituents are false, then the combination must be false.

Then, given this *interpretation* of the formula " $p \cdot \bar{p}$ ", there is at least one instance of the formula thus interpreted in the text. And since the formula is a contradiction, there is at least one contradiction in the text.

SEM: Excellent. But *is* there in the text a conjunction of one proposition and its negation?

LOG: There is the sentence "Norway is a democracy and Norway is not a democracy".

SEM: Is there?

LOG: To be more accurate, there are the two sentences "Norway is a democracy" and "Norway is not a democracy". I skipped one step in the deduction.

SEM: The deduction?

LOG: If p is asserted and q is asserted, we can infer the assertion of $p \cdot q$. It is asserted that Norway is a democracy. It is also asserted that Norway is not a democracy. This entails the assertion that Norway is a democracy and not a democracy. And this we write: "Norway is a democracy and Norway is not a democracy"

SEM: I know that if at some place earlier than j in a deduction there is a well-formed formula P , and at some other place earlier than j there is a well-formed formula Q , then at place j one may write $P \cdot Q$. Such are the rules of the game. But who told you that the author of our text is playing the same game?

LOG: He must.

SEM: I agree that he must in order for you to be able to make that deduction. It is perhaps even advisable to stick to conventions which make it possible for us to make such deductions. But that our author *does* stick to such conventions is only a probable hypothesis.

LOG: I guess you are free to talk about 'only probable hypotheses'. What you actually do is to prescribe a given role (out of several possible ones) for the sentential calculus. [SEM (to himself): what you actually do when you say "what you actually do is to prescribe a given role (out of several possible ones) for the sentential calculus" is to prescribe a given role (out of several possible ones) to my words]. When propositions take the place of the " p "s and " q "s, you turn the rules and axioms of the calculus into hypotheses about the presence of such rules and axioms in the natural language, or some specified part of it (the part being, e. g., certain types of situations wherein the language is used, e. g., scientific communication).

One usually gives the calculus a very different role. So when I say that I deduce an asserted conjunction p and q from the assertion of p and the assertion of q , I do not make any hypotheses about the rules or conventions which our author may happen to follow (on whatever basis we decide what those rules are (in whatever sense of "rule")). I make a simple inference of logic. A rule of inference like, e. g., $P, Q \vdash P \cdot Q$ is not

restricted to the sentential calculus as a formal system, and applicable to a particular text only if it is empirically true that the rules of inference for the calculus are isomorphic to the rules of inference used in that particular text. Hence, I don't hypothesize that the logic of the text is isomorphic to the propositional calculus — rather, I announce that it shall be so.

Since this legislation is in fact accepted by quite a few people, there are quite a few people who accept the deduction of *p* and *q* from *p*, *q*. If a given author follows different conventions, well, then in a sense it is not *his* text we read when we read what he has written. (And we are under no *obligation* to read *his* text either.) The text may be more consistent in his logic *i* than in our logic *j*, or it may be less consistent — if you don't postulate that no text is inconsistent.

SEM: I don't suppose you mean to imply that the isomorphy between, e. g., Boolean algebra and electrical circuits is normative. Yet, it is, perhaps, better to take the isomorphy between, e. g., the sentential calculus as a formal system and the logic of (a particular application of) natural language as normative. Hence I withdraw the first candidate in my list of empirical presuppositions in your little piece of logical analysis.

2 Before I present the second candidate, let me make a comment on your very last remark. — I don't postulate that a text is never inconsistent. But I do postulate that a philosopher or scientist never *intends* to assert a contradiction or a tautology. Take the latter. (And by "tautology" I mean the same as "sentence which, whatever its form, expresses an analytic proposition".) There is, perhaps, one way in which a tautology could be said to inform, namely by *exhibiting*, not speaking about, some definitional property of its logical subject (if the proposition is of the subject-predicate form). In order to do so, however, it must be recognized as a tautology offhand (if not, it can be judged to be one only by presupposing the information it was intended to yield). But how can one know offhand whether a given sentence is intended as a tautology or not? It may have some visible mark signaling its status. But then it is synonymous to, or equivalent in function to, a sentence of the type

"In our terminology the sentence ". . ." is a tautology", which is not a tautology.

Tautologies proper give us no information whatsoever, and quasi-tautologies (i. e. marked tautologies) give us only indeterminate information about definitional properties. Whereas a given definition determines uniquely a set of tautologies, a given tautology is determinable by more than one definition. (If I know that the term "east" within a given text is synonymous to "where the sun rises", then I know that, within that text, each specimen of the sentence "The sun rises in the east" is a tautology. But if I know that the sentence "The sun rises in the east" is a tautology, do I then know *what* it is which has at least one definitional property exhibited? (Is it the sun, or the rising of the sun, or the east, or what?))

The postulate (or rule of interpretation), then, is: To the extent that a given author intends to inform, he does not intend to assert tautologies proper (since tautologies proper do not inform). (So there is a sense in which a mathematical formula does not inform — and there is a sense in which it does, e. g., if it is interpreted as a statement about itself; just as a given position in chess can be interpreted as a statement about itself, that one can reach it from the opening position by legitimate moves.)

However, a given text may contain *un-intentional* (or *un-noticed*) tautologies or contradictions. Neither of the two players in a game of chess intends to play his king into a position of checkmate, and yet, checkmate is the usual ending of a game.

So I take into account the possibility of contradictions. Only we ought to notice that when we identify something as a contradiction, we presuppose a number of empirical premisses to be true. Returning to your particular analysis: If our author asserts *p*, and also asserts *q*, then we agree to make him responsible for the assertion of *p* and *q*. But (and this is my second candidate) how do we know that he asserts *p*, and that he asserts *q*? You take it for granted that he asserts both of them. But perhaps he does not. Perhaps he asserts only one of them.

LOG: There is of course the chance that I overlooked some quotes, i. e., that one of the sentences is *mentioned*, not asserted.

SEM: I think I ought to reformulate the point I am trying to make. One classification of empirical sentences (or propositions) is into *observation-sentences* and *hypothetical sentences* (universal or particular (synthetic) sentences that refer to non-observables). The distinction, as I know it, is rather vague, i. e. on a number of relevant points the decision mechanism of the definiens (ignoring at the moment the multiplicity of definientia) does not react — and either one is not able to sort *each* sentence, or one sorts on the basis of *ad hoc* decisions. But the vagueness of the distinction does not affect its job here. It is quite clear that you do not *see* the meaning of a given word. I mean, the meaning is not to be found printed on your retina as is (in some sense) the physical shape of the word. A sentence about the physical shape of a word is therefore classified as an observation-sentence, whereas a sentence about the meaning of a given word is classified as an hypothetical sentence. (This is a rough way of dealing with delicate matters, but the topic of *seeing* is only peripheral to our discussion.)

Well, then my point is: *In order to establish the result of your logical analysis as a conclusion in a deduction from premisses, you have to include in your set of premisses sentences which are not only empirical, but also non-observational, i. e. hypothetical.*

From your experience with chess and logistic symbol games you know that it is possible to overlook an illegitimate move. [As to logistic games, a statement that a particular sequence of well-formed formulas is, in fact, a proof may be construed as a conjunction of observation statements — if the rules of inference are interpreted and applied, but not spoken about. (The restriction is not arbitrary. For one thing — and apart from more serious consequences — if a statement about the meaning of a sentence S is taken to be included in every sentence S, then there exist no observation-sentences.)] You therefore reckon with the possibility of *observational* errors. But the premiss that both *p* and *q* are asserted is not an observation-statement. “*p* is asserted” does not (or ought not to) mean the same as “*p* is not enclosed in quotes”. However we explicate the meaning of “*p* is asserted”, the explication ought to be restricted so as to make it true that

the absence of quotation-marks (or other observables) does not guarantee that a sentence is used (i. e. asserted) — and also to make it true that the presence of quotation-marks does not guarantee that the sentence is mentioned.

LOG: You have a point there. There *is* a difference between saying about a particular specimen of the well-formed formula " $p \cdot p$ " that it is a contradiction, and saying about a particular specimen of the sentence "Norway is a democracy" in conjunction with a particular specimen of the sentence "Norway is not a democracy" that it is a contradiction.

I guess it is (at least in part) a matter of choice whether one says that the term "contradiction" does not mean the same in the two cases, or that the two sentences of the form " p is a contradiction" mean the same, but that the second one is harder to verify.

SEM: You now talk like a professional semanticist. Yet, since it is I who play the semanticist's part in this dialogue, let me play out his arguments — if not for your sake, then for the sake of our invisible spectators.

Not only does a logical analysis like our example include premisses of the type " p is asserted". It also includes two other types of empirical — and hypothetical — premisses. The one is about those parts of the text that are symbolized by *sentential connectives*, the other about those parts that are symbolized by *sentential variables*.

4 A logical analyst cannot escape premisses about the relation between, say, a given instance of "or" and the usual semantical interpretation of the wedge, i. e. he cannot escape hypotheses about the probable meaning of "or" in a given context if his analysis is to be of *the text*.

If an analyst says, "I do not assert that this particular "or" is *in fact* inclusive, or probably so, only that *if* it is, then it can be symbolized by the wedge" — *and then symbolizes it by the wedge*, then either he does assert that the "or" is in fact inclusive, or his assertion does not justify his move (if it is *the text* he symbolizes). It's like if a person says, "If p , then q ", and then concludes, " q ": either p is an implicit premiss or q does not follow.

If a sentence of the type "If p , then q " is taken to assert material implication from p to q , then it is true if only q is true. But if the point is to present p as a *justification* of q , we require that p shall be relevant to the subject matter, and also *true*.

If the analyst does not care about the actual or probable use of "or" in the text, then there is a recipe which may bring him some emotional satisfaction (*something* ought to guide his choice):

If a friend has written the text one says, "If this "or" is inclusive, then it can be symbolized by the wedge" — and then one symbolizes it by the wedge (and thus eliminates one possibility for contradiction). If an enemy has written the text one says (about the same "or"), "If this "or" is exclusive, then it can be symbolized by the symbol for non-equivalence" — and then one applies that symbol (and thus creates one more possibility for contradiction).

If an analyst does not want to create the illusion of justifying his moves, he may just say nothing or anything, e. g.: "If this "or" is a material implication, then it can be symbolized by the horse-shoe" — and then symbolize it by the wedge (if, for some reason, he sticks to the convention of symbolizing the word composed of the fifteenth and the eighteenth letter of the English alphabet, in that order, by the wedge).

- 5 LOG: Our spectators ought to notice that SEM speaks about the logical analyst whose job it is to find equivalences within a calculus to given items used outside the calculus. In his job the question of the material adequacy of a given symbolization (e. g. of representing a given instance of "if p , then q " by " $p \supset q$ ") is fundamental. With the pure logician the case is different, since the formal value of, e. g., the connective called "material implication" is independent of how well it fits with some frequent use of "if —, then —", which is how the horse-shoe is *read*, very often.

SEM: Your distinction is worth a lecture of its own. Let there be a blackboard here, and a piece of chalk, too.

Consider those situations where there is a relation implied between, on the one hand, some connective like the horse-shoe, the wedge, etc. (" \supset ", " \vee ", etc.), call it an *F-element*, and, on the other hand, some item of English like "if —, then —", "or", etc., call it an *N-element*. Within such situations there is a variety of possible relations which may hold between the two elements in the pairs which are composed of one *F-element* and one *N-element*, e. g. in the pairs " \supset " and "if —, then —", " \vee " and "or". (We do not discuss pairs like " \supset " and "or".)

Some of these situations are relevant to our discussion. Consider the following:

- (1) The *meaning* called "material implication" (i. e. the meaning attached to the horse-shoe when the sentential calculus is given its principal interpretation) is given. The job is to select some item of English to convey that meaning. (Imagine, e. g., an intelligent philosopher unfamiliar with mathematical logic saying to himself: I need a symbol to combine two sentences such that if the first is true and the second false, then the combination is false, otherwise it is true.)

Two possible responses to situation (1) are:

- (11) One adopts the expression "if — then —".
- (12) One adopts the expression "if —, then —", and attaches a recipe which tells the reader to interpret it as material implication.

In case of (11) one relies heavily on (even if one does not explicitly assert) the hypothesis that "if —, then —" is normally (within the relevant sort of context) used and interpreted in the sense of material implication. — In case of (12) one probably relies on the hypothesis that, of existing alternatives, "if —, then —" is closest (in some sense) to material implication.

A logician is sometimes in situation (1) when he tries to formulate professional assertions in English. Notice that English terms enter even into his axioms and theorems.

- (2) The expression "if —, then —" occurs in an English sentence. The job is to give it a materially adequate symbolization within the propositional calculus.

Possible response:

- (21) One symbolizes it by the horse-shoe (interpreted as material implication).

As in case (11) one relies heavily on an empirical hypothesis about the relation between "if —, then —" and material implication. The weakest hypothesis which if true justifies (21) is: the present instance of "if —, then —" is used in the sense of material implication. But the verification of this particular hypothesis is not independent of the verification of some general hypothesis like the one indicated in the comment on (11) above.

(2) is the situation of our logical analyst.

- (3) The horse-shoe occurs *as an interpreted symbol* within the propositional calculus, e. g., in the formula " $p \supset p$ ". The job is to desymbolize the formula, or at least the horse-shoe.

Possible response:

- (31) One desymbolizes " $p \supset p$ " as "if p , then p ".

The hypothesis implied is like that of (11), though situation (3) is not quite like situation (1). In (3) it is a symbol and the meaning attached to it which is given, in (1) it is the meaning only (irrespective of the shape of its symbol, and irrespective even of the existence of any (single) symbol with that meaning).

- (4) The horse-shoe occurs as a symbol in the formal system called "the sentential calculus" or "the propositional calculus". The job is to give an oral presentation of the system.

- (41) The horse-shoe is read "if —, then —" (e. g. " $p \supset p$ " is read "if p, then p").

Case (4) is quite different from the cases (1) — (3). In (1) — (3) some *synonymity-relation* is implied (i. e. asserted by implication) to hold between the F-element and the N-element. In (4) the N-element is used either as a *name* of the F-element (insofar as the oral presentation of the system consists in talking about it), or it is used simply as a *reading device* (insofar as the presentation consists in oral reading of symbols and formulae): the N-element (as an item of the spoken language) is the *associated reading* of the F-element. (Human beings do not seem to be happy with logistic symbols unless some pronunciation has been matched to each of them. Even dots and dashes are read: "dot-dot-dot-horseshoe-dash".)

One's *motives* for choosing the reading "if —, then —" rather than "or" or "horse-shoe" may differ:

- (a) because one believes that "if —, then —" is usually, or correctly, used in the sense of material implication;
- (b) because it is usual to pick the reading from the vernacular, and one believes that the meaning of "if — then" (in some of its frequent uses) is closer to material implication than is the meaning of other vernacular terms (like, e. g., "or", "and");
- (c) because it is usual to read it like that, and one does not want to complicate matters by introducing variant readings.

Whatever one's motives for choosing a particular reading, the very reading does not imply the assertion of any synonymity relation.

When the vernacular N-elements are used as names of logical connectives their status is similar to that of *test-names in psychology*: they are short labels which can be read without special instruction (since they are from the vernacular), and which have some mnemo-technic value since, we assume, each of them has been matched to the connective most similar to it in meaning (i. e. when the connectives are given their usual interpretation).

This, of course, is not the whole story — either for the test psychologist or for the pure logician. For the latter, who is not responsible for any particular interpretation of a formal system, there is still the theoretical question of the nature of such an interpretation.

LOG: I can think of an analogy which, even if it does not reflect the network of distinctions in your lecture, still takes care of a major point.

Just as a painter may be abstract or naturalistic in relation to a given model, a logician may be abstract or naturalistic in relation to a given text. Very few propositions about the visual properties of an abstract painting are true about the visual properties of the model. But the absence of isomorphy between painting and model is no imperfection of either. If a series of English sentences is used as a stimulus in writing down a series of well-formed formulas, it is no defect of the formulas if they are not logically isomorphic to the English sentences (i. e., when the formulas are given their principal interpretation and the English sentences are given their probable meanings). Nor is it a defect of the English sentences. But a naturalistic portrait is imperfect if it does not resemble its model, and a logical analysis of a given text is defective if each well-formed formula is not isomorphic to the sentence it symbolizes.

6 SEM: Well, then my point is: In order to verify the existence of logical isomorphy between text and symbolization (take this wording in the sense you gave it above), one has to verify a number of empirical propositions about the actual meaning of the sentences in the text. We have discussed the premisses about sentential connectives. The same sort of argument applies to our use of sentential variables. Our premiss is then:

Sentences which are symbolized by the same sentential variable are synonymous, and sentences which are symbolized by different sentential variables are heteronymous.

We do not know *a priori* that two specimens of the same sentence are synonymous any more than we know *a priori* that

two specimens of different sentences are synonymous. I. e., our concept of 'synonymity' ought to be such as to imply what I just said. In the terminology of empirical semantics a given *term-type* is not *per definitionem* synonymous to itself. To make it so would not exactly amount to a denial of ambiguity, but it would collide with (other) existent terminologies in which the same term may be said to be used in different senses. A given *term-token* is synonymous to itself as used or interpreted by a given individual P_i at time t_i . Once P or t is varied, the interpretation of the given term-token is open to variation.

Hence, in order to justify the verdict "contradiction", it is not enough to cite two sentences (e. g., from the same text), and to point out that if the one has the form p , then the other has the form *not* p , since it is identical to the first except for a "not", etc. The observable properties of a pair of sentences furnish sufficient evidence only if the verdict is about observable properties (which it is not), or if there is an approximate one-to-one correlation between meanings and observable properties (which there is not).

The same is true for the verdict "tautology" or "analytic". A sentence of the form "S is analytic" is empirical — and hypothetical — whether S be a particular specimen of the sentence "No bachelor is married" or a particular specimen of the sentence "No unmarried man is married". The *meaning* of the sentence "S is analytic" is as clear or as unclear in the one case as in the other. The notion of synonymity is equally involved in both. On an intuitive interpretation of "S is analytic" I find the *likelihood* of analyticity greater for a given specimen of "No bachelor is married" than for a given specimen of "No unmarried man is married". My verdict is based on the belief that *whenever a given sentence is conspicuously analytic, it is probably not analytic* — making the thesis itself an example (almost) of what it asserts. It is my guess that research in empirical semantics will verify it.

ELSE FRENKEL-BRUNSWIK †

By the death of Else Frenkel-Brunswik psychology and social psychology have lost one of their pioneers. And this journal has lost an advisor from whose work and stimulation the journal would have greatly benefited.

At the end of the war against Nazi-Germany we were impressed by a rumour from USA that a group of psychologists and social-psychologists in USA who were working with a notion they called 'the anti-democratic' or 'the authoritarian personality'. The notion had been created in connection with their attempts to explain why some people accepted ethnocentric or fascistic ideologies. Their research seemed to show that some of the causes might have to do with deep-lying personality-traits, especially with the way in which the individual reacts towards his own weakness and aggression.

The contact with this line of research became for us something much more important than a mere satisfaction of reading a fascinating research-report. It influenced our thinking, our research-interests and our view on life in general. And we wanted to get to know the people in this group. Especially we wanted to be acquainted with Else Frenkel-Brunswik, since we knew that she — through her close contact with the Vienna-circle — had a philosophical orientation and was interested in that combination of philosophical analysis and empirical research which seemed so fruitful to many of us.

From 1946 and on, several Norwegian scientists and philosophers found their way to Berkeley, California, to meet her and to learn from her. Later on she came to Oslo in order to help us with our work.

She represented a unique combination of qualities. In her research she was daring, creative and original. Her combined use of modern statistical survey-techniques and Freud's theories has opened up a whole new direction of research. Her integration of theories of perception and of motivation is of great theoretical importance.

However, she never did research for its own sake. She always concentrated on problems of human significance. Her first consideration was not: Can this problem be attacked by the use of reliable statistical methods? But rather: How theoretically significant and humanly important is this problem? Methods and statistics were not the decisive thing.

She was so warm, so generous and giving. Wise and tolerant. I remember the first time I met her. I was a little scared by what I knew about her work. But the emotional security she had, she was able to convey to others. We talked about principles of concept-formation, value-questions, politics and the meaning of life. Her intellectual enthusiasm was most stimulating and encouraging.

Last time she visited Oslo was in 1956—57. She was just as generous with her time as before, just as helpful and stimulating to all of us.

She had so many plans. What she now wanted more than anything else was to devote her energy to a work on human values, integrating her philosophical and empirical knowledge.

And now she is dead.

H. O.

CLARIFYING VALUE JUDGMENT: PRINCIPLES OF CONTENT AND PROCEDURE

by

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In the following pages I describe and weigh the role of two modes of clarifying value judgment. The first emphasizes what is said. The aim is to improve the adequacy of definition, of rule making, of propositional analysis. The second is relatively indifferent to content; it is in fact often regarded as a substitute. The hope of many of those who cultivate the second approach is that the mind can be prepared for eventual illumination by the practice of appropriate methods of a "non-logical" character.

Perhaps I should say at once that I reject the validity of either approach taken separately. Each is indispensable to the other. While it is true that the two have not been harmonized in the past, the contemporary climate appears to be more favorable to the growth of a unified interpretation.

It may seem that with the choice of terms like "judgment" and "clarification" the cards have been stacked in favor of the analysis of content, since these words have usually been appropriated by the historical champions of "reason". Unmistakably they exude the aroma of the Enlightenment; but I want to put the reader on guard. It may suffice to say that the term "judgment" is not definitionally restricted to such subjective events as "facing a problem" or "expressing a preference". Besides "mental events", an act of judgment is taken to include both "bodily" and "behavioral" events.

A different choice of words would save us from at least some misinterpretations. Unhappily another choice would create another set of misunderstandings. If we use words with histories, they have already shaped some of the predispositions now employed in their interpretation. If the words we use are entirely fresh — a brand new slate of non-sense

syllables — they run the risk of contamination by unwanted meanings at the moment that they acquire acceptable ones.

The Content Approach

A particular judgment may be regarded as a typical point of departure for the analysis of content. Such an operation is a subjective sequence of events; in it a preference is affirmed by a statement maker. We may take the following as a representative “thought”, “meditation” or “rumination”: “I am in favor of strengthening the United Nations” (or, “I am against it”).

A statement of the kind can be elaborated in two directions. The maker of the statement may justify his thought in more comprehensive terms: “The UN is the nearest expression today of the ideal of a commonwealth of man; all history is a struggle toward world commonwealth, and I am on the side of unity and progress.”

The maker of the statement may specify the original proposition in more detail: “I do not mean by the UN all associations with which it has formal affiliation; nor do I refer to the Council or the Assembly as such. I am talking about the formal obligations of the Charter, and especially the renunciation of aggression.”

The first line of elaboration we call *grounding* a judgment; the second, *specifying* a judgment. It may be observed in passing that the semantic hazard is still with us, though in diminished degree. What we describe as “grounding” has quite different connotations from the earthbound activities usually associated with foundation building in people’s minds. Perhaps it would be more acceptable to visualize the clarifying of value judgment as an expedition that passes along a road that forks in two. One fork leads to the uplands of abstraction, often disappearing in mist and cloud beyond the eye of empirical inquiry, while the other descends toward low country where it is easy to specify the foothills, villages, plains, and beaches.

Empirical Grounding

The statement cited above in favor of the UN grounds the position taken by invoking terms like “history” and “progress”. But it is not obvious whether “history” refers to past “trends” exclusively, or whether it is also supposed to designate a factor that determines the alleged trend. In the latter case the statement maker would presumably be willing

to formulate scientific hypotheses. Thus: "Progressive forces are influencing world affairs toward unity"; "Progressive forces have been strong enough to overcome reactionary forces and to move the equilibrium of world affairs toward unity". (Before serious research could be done on these propositions, of course every key term would call for operational definition.)

It is worth noticing perhaps that we would have been in doubt about the intention of the speaker if he had talked of history and progress alone, without declaring himself categorically in favor of progress and unity. Had this declaration been left out, his position would have been "normatively ambiguous". That is, in classifying his remarks we would have reflected that the speaker is Western European and that Western Europeans usually speak well of "progress". Having no unequivocal declaration, however, we would have had to admit that the speaker's stand remained in doubt. By suitable investigation we might discover that he loathed "Progress" even as it rose to dominance.

Our analysis up to this point may convince us that the speaker intends to ground his preference for the UN upon the authority of his own affirmation of the general proposition that unity is good. This is *grounding by authority of the ego*.

The ego's preferences are empirical events; that is, they are capable of being observed with the ordinary psychophysical equipment of man. Observation "of the Self by the Self" is among our ordinary means of obtaining empirical data.

(A variation upon the empirical mode of grounding a judgment is the citing of *another individual or group* as the authority. This however need not detain us further here.)

Transempirical Grounding

In direct contrast with empirical grounding is the invocation of authorities who are declared to be beyond the reach of human understanding. Suppose that progress is said to be in harmony with Divine Will; that the historical trend toward unity is in harmony with progress; that Divine intervention has been on the side of unity; that future intervention of the same kind will make unity inevitable; and that the statement maker prefers to be in harmony rather than in opposition to Divine purpose. Assume, further, that these assertions are described as acts of faith; and that it is freely conceded that empirical knowledge is insufficient to substantiate many, and perhaps most, of the statements uttered. We are

in no doubt about the transempirical grounding of whatever judgments are supported by these statements.

Masking the Transempirical

There is, however, a marginal case. I have in mind a situation in which the thinker denies that he is making a transempirical imputation. Perhaps his position is this: "History has guided mankind toward Progress in the past, and will continue to do so in the future. Regardless of my likes and dislikes, world unity is inevitable. My choice is to work with winning, not losing forces; I choose progress."

That the speaker has gone beyond the available stock of empirical knowledge is obvious. By declaring a particular version of the future to be inevitable he substitutes an affirmation for an estimate. Hence we must classify the words according to categories that differ from the manifest content. The statement maker has gone beyond the characteristic limitations of the empirical and given credence to transempirical authority, though without acknowledgment.

We are now beginning to tread the familiar uplands of theology and metaphysics. However, the foregoing distinctions are not our principal concern, since we want to develop a procedural rather than a content approach to the grounding of value judgment.

The General Significance of Procedure

Traditional critics of the "rational" approach have always felt cramped and stifled by it. Truth by syllogism — more generally, truth by definition — rings hollow in the ears of all who set themselves the task of capturing the direct experience of "truth itself". Their aim is to dissolve the screen that appears to separate everyday events of feeling and perception from the universe of the transempirical. Such minds are in perpetual protest against the ceiling that our psychophysical apparatus imposes. The moment of transcendence — characterized as the vision of the face of God, or the blue luminance of cosmic integration — is the mystery toward which every moment flows.

The mystic tradition incorporates a point that we accept as of basic importance in the pursuit of rationality. By what procedures can the mind be made most fit for rational clarity? Content analysis can itself be viewed as a procedure, since time is required. But the usual mystic rejects the analytic procedures of logic as too restricted in scope to

encompass the vast enterprise of truth. He is consciously contemptuous of time; his aim is conceived to be the eternal. Yet on examination he is curiously respectful of time. He is aware of the heavy investment — of hours, of months, of years — likely to go into the preparation of a state of readiness to receive illumination. Such a preparation can worthily absorb the whole of life.

Two facts stand out when we examine the spiritual exercises of mystic cults: the individual's *focus of attention* is trained; his *non-subjective dimensions* are also trained. Many of the leading schools aim initially at exhausting the events offering themselves at the focus of attention; they cultivate freedom from images of perception or from the coloration of mood. Emptiness is regarded as an acquired precondition of vision.

Training is not limited to the management of subjective events by subjective means. On the contrary, the interdependence of mind-body events is fully recognized; and body exercises attune the soma and the psyche. Not the least impressive outcome of some training is the automatic response of the whole psychosomatic system to refined cues in the environment.

A number of factors appear to be creating a receptive attitude among Western European thinkers for procedural approaches of the kind that have prevailed for so long in the East. There is dissatisfaction with what can be accomplished by the further pursuit of preciseness. Professor Naess has provided abundant documentation of the fact that the quest of preciseness always seems in some sense imprecise. Some fish always get through the logical net, and it is by no means obvious that a further colossal expenditure of energy on improving the net will produce significant increments of yield in Western thought.

Over the years the psychological and social sciences have been building a frame of mind congenial to a procedural emphasis. The notion of events in time, of sequence, has brought about a redefinition of thinking, perceiving, and behaving. In describing these operations the old dichotomy between mind and body has been melting away, so that it has become plausible that categories of reference to "subjective" and "non-subjective" events are distinctions of operational convenience with no fixed empirical boundaries. In practice a mixed bag of terms is usually employed to characterize the internal and the external components of an "act".

One way to put the point is that since every act is an interaction, it includes patterns of "communication" and "collaboration". The former word is defined operationally in terms of subjective events (observa-

tions of Self by Self) and "signs". A sign is a physical resource that is specialized to the function of reference. The subjective events are "symbols"; they are "intentions" and "interpretations". A sign refers to the "symbol" events.

Collaborative activities involve fighting or building, for instance; they use resources not specialized as signs. (Non-sign resources are "materials".)

All acts can be described as patterns of material-sign-symbol events. Neurological, glandular and muscular patterns are among the material components; standardized sounds and gestures are signs; interpretations of signs are symbols. The fading out of the lines that were formerly assumed to separate the "organic" from the "inorganic", or the "body" from the "mind", leaves an all-encompassing manifold of events in which any observer-participant is free to use all convenient operational indexes to describe any event of reference.

It begins to appear that "content analysis" as a means of rationality is a bootstrap operation, since in examining content the ego is kept within the limits of conscious experience. The statement maker is thereby restricted to memories of the kind that enter conscious awareness with ease. It is well known that the technique of analyzing definitions and criticizing propositional statements does not enable the ego to obtain access to the "unconscious". The conscious process itself may be under the domination of repetitive compulsions which are outside the awareness of the thinker. If rationality includes the notion of freedom of choice and hence of freedom from obsession and compulsivity, it is obvious that rational thinking requires the use of appropriate procedures by means of which the thinker obtains access to pertinent facts about the Self.

The technique of free association (or any of the projective methods devised since Freud) has a different aim than the spiritual exercises of the mystics. The principal purpose of free association is to increase the supply of intelligence available to judgment. Spiritual exercises, on the contrary, typically eschew information, since they seek to empty the mind, to create an uncluttered screen where a transcendent experience may eventually occur.

I am impressed by the fact that the prominence of psychoanalysis (and related techniques) has diverted attention from other developments in modern times of no less importance for a procedural approach. What the thinker can accomplish by the use of logical technique is circumscribed by the cultural tradition in which he is brought to maturity. We

have been learning from the late Professor Whorf and others about the pervasive influence of linguistic forms in determining categories of thought. Evidently the thinker's potential for rationality increases when he becomes acquainted with the impact of these factors upon the Self.

Within the orbit of a given culture modern social scientists are describing the environments in which upper, middle and lower class practices are stereotyped in people's lives. They are also describing the role of special interests in shaping the perspectives and operational activities of those who are exposed to them.

More significant perhaps is the implication of social scientists that too much emphasis has been put upon "the individual" as the significant unit in society. We are learning a great deal about the fragility of most human beings when they are removed from supporting environments. Quite recently the subject was dramatized by the phenomenon of "brain washing". But cumulative observation points to the possibility that human affairs would be better understood if they were analyzed into "situations" or "interaction sets" composed of the value standards (and specific interpretations) which are recognized and sustained in the continuing interplay among participants in the social process. No one person characteristically dominates or even defines and perpetuates these networks. Indulgences and deprivations (in terms of affection, respect, enlightenment, and other value outcomes) are stoutly intertwined. It appears that societies are composed of untold thousands of such situations of interaction. Individuals simultaneously participate in a great many, typically moving in and out with a minimum of impact upon each. The clarification of value judgment appears, in this perspective, as an "interactive" rather than a "privatized" activity.

Configurative Thinking and Representative Exposure

Procedural principles appear to concern two problems: One, how to employ the assets in hand in the task of judgment; two, how to enlarge the range and quantity of assets at one's disposal. I refer to the use of current assets as "configurative thinking", and the expansion of scope as "representative exposure".

Configurative thinking is goal oriented. It is contextually oriented in the sense that not one but all goals are considered. It is also oriented in time, since value judgments are choices in the emerging process of history. It is oriented toward knowledge, since all available information

is taken into account. In a word: configurative thinking is goal oriented, time oriented, knowledge oriented; and it can best proceed by employing five interconnected though distinguishable patterns of thought.

Goal Thinking

The proper emphasis in goal thinking is upon the *contextual whole*. We do an insufficient job of clarifying a *specific* commitment to the UN, for instance, if we fail to assess it in relation to *all* of our *general* goals. We do an inadequate job if we fail to appraise *each general* statement in reference to *all*. A commitment to world unity, for instance, requires qualification if we are also committed to political freedom. *Unless tentative value judgments are reviewed in the context of a total conception of the preferred form of social order, unnecessary inconsistencies and omissions occur.*

Goals are long range, mid-range, and immediate. The long range ("Utopian" goals) are almost independent of timing. But little of any consequence can be said about mid-range and immediate objectives without giving full weight to temporal ordering.

Trend Thinking

Herewith the time dimension is brought directly to the thinker's focus of attention. He asks himself in regard to a given context to what extent preferred goals have been approximated? How much sacrifice has been made in order to achieve them?

Scientific Thinking

Although trend knowledge is helpful in obtaining preliminary estimates of value cost and gain, scientific analysis is essential to refine the initial estimates. It is customary of scientific thought to include the construction of explanatory descriptive models, and the gathering and processing of data. It is well known that historical trends may create misleading impressions of the strength of various factors that condition change. Perhaps great results appear to have been achieved by personal leadership and negotiating skill. Scientific analysis may show that in the concrete circumstances these factors were of trivial importance when compared with the impact of "atomic stalemate".

Projective Thinking

Trend and factor (scientific) thinking are requisite to projective thinking, which is concerned with future events. The question is, if we assume that no policy changes will be introduced, how will things turn out? Projective thinking is carried on by the use of "developmental constructs", which we characterize as theoretical models of significant cross-sections of past and future. Goals (preferred events) are taken as points of departure for these constructs. Hence one may begin with world unity as exemplified in the UN, starting with the inauguration of the UN after World War II and selecting ten or twenty year intervals in the future which are then characterized in terms of a strengthened or weakened UN.

A developmental construct is formulated on the basis of all information at the disposal of the thinker; and it is subject to critical reconsideration as information improves. For developmental purposes information is of three kinds: systematic historical (trend) knowledge; scientific propositions (with a specifiable degree of empirical confirmation); unsystematic information about the past and present.

Obviously it is not enough to extrapolate historical trends into the future, since in many cases the curves come into conflict at future points; and trend knowledge provides no method by which the result can be forecast.

Further, it is not enough to project scientific generalizations into the future. Scientific propositions avoid time; they treat time, where it enters, as a factor whose routine of interaction with other variables (at a specified magnitude) is *invariant*. Thus time is considered in order to escape from its role in the "natural order" of past and prospective history. In this perspective the mystic and the scientist share a curious ultimate bias against time. Developmental thinking, on the contrary, stresses "natural order", and refers to the future in terms open to validation by future observers. Since scientific propositions are formulated as invariant relations the task of projective thought is to estimate the likelihood that the stipulated conditions of invariance will appear at specific future times.

Often, when one turns to available knowledge about the past, it appears that scholarship and science have failed to provide much that is directly in point. But a large amount of unsystematic information is at hand with which to fill out the current picture as best one can.

Policy Thinking

We come to policy thinking, or the invention and assessment of alternative courses of action. Instead of assuming "no influence", as was done in projective thinking, the situation is examined to disclose ways and means of influencing the future as it unfolds. Hence projective thinking is continued in a different frame of reference. Stress is laid upon inventiveness, upon permitting new ideas to enter into the historical process through the subjective life of the thinker.

For relatively circumscribed problems it is feasible to draw up maps of preference and expectation, after the manner recommended in game theory. We are, however, concerned with the social context as a whole, and it is beyond the present range of possibility to draw comprehensive maps of preference and expectation. Sufficiently precise operational indexes are not now obtainable. Also, the computations required by the army of potential variables are not practicable for existing equipment.

A cautionary note: The impression may have been given in the present discussion that configurative thinking is like a ceremonial dance that follows a strict progression through a series of regular figures called goal, trend, scientific, projective, and policy thinking. However, no "once and for all" implication is intended. Procedural flexibility improves the chances of clarity, since it is possible to employ each method until the marginal probability of further contributions by each is estimated to be the same.

Representative Exposure

We have said that the configurative method can be employed to utilize the knowledge accessible to the thinker at a given time. We turn now to procedures whose principal role is to prepare the thinker to endure or attain a higher level of rationality. Once more the key conception is contextuality. We speak of representativeness; the intention is to emphasize that the ideal program is a sampling of all human experience. We know enough of learning to realize that learning is largely situational. It occurs in networks of interaction. Much of what purports to be social learning is exceedingly superficial because there has been no full participation, no saturation in the culture, the class, the interest group, the crisis situation, or even the type of personality about which the learning is presumably taking place.

The exposure program is designed with two lines of intensification in view: One, *understanding* of people; two, *insight* into the Self. The

latter factor places a formidable limitation upon the degree of penetration possible in efforts at understanding. In this connection we are using the technical conception of insight; we refer to the discovery of attributes of the Self that are ordinarily excluded from the focus of full waking attention by smooth working mechanisms of "resistance" and "repression". The anxieties are the cover forces that stand in the way of rationality.

Individual and Collective Projects

I have been describing the pursuit of clarified values as a project for individual thinkers. So it is. But individual thinkers need not try to restrict themselves to the Robinson Crusoe role which has been a favorite image of the Self among thinkers. It is out of the question for a solitary thinker to accomplish as much alone as when he is functioning as part of a system in which social intelligence operates effectively. Estimates of the future depend upon access to a current flow of information that is comprehensive, representative, concise and reliable. To the extent that the agencies of enlightenment are malfunctioning, the mind of every thinker suffers handicap. Some intellectual tasks cannot be performed.

The thinker depends upon more than the current flow of intelligence; he is nurtured in the antecedent process by which he obtained skills and motivations to pursue enlightenment as a major value.

Today the thinking group — that is, the specialists who pursue an inclusive and tested image of the whole of human history — have a more formidable task than ever. There is need of an *intermediate stratum* of thinkers to function between specialists on "truth by definition" and men of action — or desperation — who are the responsible makers of value judgments in every sphere. The intermediate role calls for the life-long cultivation of the thinker's potential for rationality.

There is continuity in such a life. At any cross-section the individual finds himself "seized" by a host of value preferences; the technique of choosing among them is improvable. The principal lines of improvement appear to be in the methods of configurative thinking and of exposure to representative occasions of understanding and insight.

The task of clarification does not come to an end in the career line of individuals or collectivities. It continues as long as time exposes a new visage in history. The act of judgment is rationally performed when further reflection would, in all probability, add no gains comparable with the losses of postponement. We are suggesting that the value commit-

ments of the individual need not be arbitrary, even though they are empirically rather than transempirically grounded. The primary reliance of the thinker in this operation needs to be upon procedures rather than upon the analysis of the content of definitions or propositions. The procedures themselves are two-fold, one addressed to the effective use of the intellectual assets at hand, the other to the improvement of the thinker's fundamental equipment. In the effective use of available knowledge the configurative pattern increases the contextuality of goal, the historical sweep of information, the scientific thoroughness of analysis, the imaginative anticipation of the future, and the inventive appraisal of policy that increase the likelihood of an optimally rational result. In the enlargement of range and diversity of understanding and insight the individual — and the group — has an opportunity for stretching human capability to its uttermost limit in an unending search for clarity of judgment.

PHILOSOPHY (I)

by

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Anyone who is a professional teacher of philosophy must be well aware that many of his colleagues regard his subject as at best extremely queer and at worst completely bogus. He knows too that this impression is not confined to stupid or ignorant or unfriendly persons, whose opinions on the matter he can light-heartedly set aside. We may usefully begin by asking ourselves what are the causes of this unfavourable estimate, and how far, if at all, they are valid reasons.

We may remind ourselves at the outset that doubts about the validity and the fruitfulness of philosophy, as hitherto pursued, have not been confined to outsiders. At least three of the most eminent European professionals, Descartes, Locke, and Kant, were moved to initiate the philosophic developments for which they are famous by their profound dissatisfaction with all that had gone under the name of 'philosophy' before their time. It is true that each of them claimed to have found a remedy and to have set philosophy at long last on the right course. But their successors can derive little comfort from this. For, whilst we cannot but admit that each of these great men was largely justified in his criticisms of philosophy as practised up to his time, we must also own that the remedy proposed by each of them has to all appearance failed to cure the disease. The philosophies of Descartes, of Locke, and of Kant seem to be open to very much the same kind of objections as these writers launched against the philosophy in which they were brought up.

These objections may be summarized as follows. (1) Philosophy claims to be a branch of knowledge or of well-founded belief. It is not content to be regarded as mythology or poetry or the expression of a personal emotive reaction. Now, if this claim were valid, we should

expect philosophy to be a co-operative work, in which generally agreed results are reached by common and accepted methods. There would, no doubt, be plenty of legitimate disagreement. There would also be a certain amount of heated and needless controversy, due to common human defects and to the personal failings of particular individuals. All this we find in other branches of human enquiry, such as mathematics, natural science, and history. But we should expect the controversies to have led to a very large amount of agreement at any moment among experts, and we should expect such agreed results to form a firm basis for further developments. Now, it will be said, we do not find this or anything like it in philosophy. At any moment we find philosophers divided into sects and schools, whose problems, methods, and very language are so alien to each other that mutual understanding is difficult and fruitful discussion and cooperation impossible. One has only to note, e.g., the coexistence at the present time of a school of logical positivists and a school of existentialists, in order to illustrate this contention. At most we may find one fashion prevailing for some years and over a considerable area, and ousting temporarily another fashion which sooner or later returns. In illustration of this it is amusing to compare and contrast the titles of articles in the famous philosophical journal *MIND* to-day and fifty years ago. At the beginning of this century many pages in each number were devoted to the discussion of intimate details in the life of the Absolute. Nowadays it is rare indeed for the very phrase 'the Absolute' to occur in a number of *MIND*, except perhaps in a book-review. Now no-one in his senses would allege that, e.g., Bradley and Bosanquet and McTaggart and Royce were any less able or learned or devoted to their subject than the leaders of English and American philosophy to-day. The inference which suggests itself is that there is something radically wrong with the subject.

It is worth while to remark that Descartes and Bacon quite fairly brought a similar objection against the *natural science* which had prevailed up to their time. They thought that the defect could be remedied by the introduction of certain new methods, and the subsequent history of natural science has abundantly confirmed their anticipations. Descartes, who had himself invented a method which revolutionized the treatment of geometrical problems, had similar high hopes for philosophy. It must be admitted that these hopes have been completely disappointed. Yet we can hardly be surprised that the wonderful transformation wrought in geometry and in physics by the introduction into each of an appropriate new method should have encouraged in other philosophers too,

such as Locke and Kant, the belief that their own subject might in a similar way be 'set in the sure path of science'. The repeated frustration of such hopes strongly suggests that the analogies which seemed to justify them were illusory. Either philosophy is not a branch of knowledge at all, or, if it is, it is radically unlike both pure mathematics and natural science.

(2) I think that the considerations which I have outlined constitute the main cause of wide-spread dissatisfaction with philosophy. Critics who are familiar with the history of scientific thought in Europe may be inclined to reinforce these objections with the following reflexions. They will say that the various branches of human knowledge which are now recognized sciences, e.g., astronomy, mechanics, and formal logic, began by being parts of philosophy. So long as they remained in that state they exhibited all the characteristic defects which have been noted above. But sooner or later the appropriate concepts, methods, and principles were discovered in each case, and thereafter the subject began to progress and at the same time ceased to be a part of philosophy. It might be alleged with some plausibility that we can see this process taking place in our own day with psychology. Does not this strongly suggest that philosophy is not a genuine branch of human knowledge at all, but is simply the ever-diminishing field within which baseless speculation and profitless controversy still have free play because the appropriate conditions for scientific treatment have not yet been elicited?

(3) If one considers the way in which philosophy is taught to students, and contrasts it with the methods of teaching in subjects which are admittedly genuine branches of human knowledge, one may well be confirmed in these suspicions. A considerable part of the training of a student of philosophy consists in studying the writings of eminent philosophers of the past, such as Plato, Aristotle, Descartes, and Kant. But it is no part of the training of a mathematician or physicist to read and criticize the works of Archimedes, of Newton, of Faraday, and of Maxwell. The methods and results of these great men, so far as they have proved to be valid and useful, have been embodied in the science and now appear in simplified and improved form in contemporary textbooks. If the original writings are now studied at all, this is only because of their historical interest as milestones on the road of mathematical or physical progress. Does not this peculiarity in the method of training students of philosophy reinforce the suspicion that the subject is not a branch of human knowledge but only a collection of *obiter dicta* dressed up in argumentative form?

Many professional philosophers nowadays would be ready to admit that there is a great deal of truth in these contentions, and would be content to make very modest claims for their subject. An influential contemporary school, with many very able adherents in England and the U.S.A., would reduce philosophy to the modest task of attempting to cure the occupational diseases of philosophers. In their writings the word 'philosopher' is commonly used to denote the holder of some opinion, or more accurately the utterer of some sentence in the indicative mood, which the writer regards as characteristically fatuous. If this is what one thinks about one's own occupation, it is certainly *honest* to announce the fact. It is not for me to judge whether it is altogether *prudent* for professional philosophers thus publicly to proclaim that their business is to take in and wash each other's dirty linen. Nor will I speculate on how long an impoverished community, such as contemporary England, will continue to pay salaries to individuals whose only function, on their own showing, is to treat a disease which they catch from each other and impart to their pupils.

What in older idioms were called 'philosophical problems' are often referred to by such philosophers as 'worries', with the implication that they are comparable to the pimples and emotional upsets of adolescence. The 'treatment' proposed for these 'worries' ranges from the agonized soul-searchings of the late Professor Wittgenstein and his pupils in Cambridge to the avuncular man-of-the-world good sense of Professor Ryle in Oxford. We might perhaps respectfully summarize the latter prescription in the phrase: "I've been through it myself in my time, my boy, and I know that it hurts like Hell; but *I* came through, and so will you if you'll only look at it sensibly!"

On the other hand, philosophers nowadays find themselves repeatedly admonished, not only by well-meaning non-professionals, but also by some of their professional colleagues, to be up and doing and to deliver a message which shall restore to humanity its lost spiritual values and save civilization. How gladly would I deliver such a message, if I had one which I believed to be well-founded, and if I saw any reason to think that it would not be either ignored or misunderstood or deliberately suppressed or distorted where it was most needed!

Those who make this demand may be invited to consider the following facts. In the first place, even if it be part of the business of philosophy to produce a *Weltanschauung*, it is no part of the business of any philosopher to produce one on demand in order to stop a leak in the ship of civilization. Secondly, there is no guarantee whatever that an honest

attempt to philosophize will lead to a view of the world and of human nature and society which liberal-minded contemporary Western Europeans and Americans would find to their liking. Plato, Spinoza, Hobbes, and Hegel were all great philosophers who had thought very deeply on these high matters, and their philosophies constitute fairly impressive instances to the contrary. Lastly, we may remind ourselves that the cruelty and violence and loss of liberty, which now prevail in a great part of the world, are the products of a fanatically held philosophical *Weltanschauung*. If we want to find an historical parallel to them, we have only to go back to the seventeenth century in Europe. Now at that time all those philosophical doctrines, to the decline of which our present troubles are often confidently ascribed, were in full bloom. Almost everyone that mattered believed quite seriously in the existence and providence of God, in the immortality of the human soul, in an objective system of moral law, and in rewards and punishments in a future life. The prevalence of these elevating and consolatory views of man and the universe did not prevent the sack of Magdeburg or the reduction of parts of central Europe to starvation mitigated by cannibalism. Indeed, if these doctrines had never been heard of, the combatants in the 30 Years' War would have needed to seek, and would doubtless have found, other pretexts for fighting and persecuting.

I think that we may at once concede the following points to the critics of philosophy. The facts adduced by them are largely genuine. These facts do make it certain that philosophy, as hitherto pursued, is not a science or a collection of several sciences, either in the sense in which pure mathematics is so, or in the sense in which physics, chemistry, biology, etc., are so. Some of these facts, moreover, make it almost certain that philosophy never can become a science in either of these senses, and that hopes that it may do so, or regrets that it has not done so, are based on a complete misunderstanding of its nature.

Whilst accepting all this, I would like before going further to make the following three remarks. (1) We must distinguish between being *non-scientific* and being *un-scientific*. What I have admitted is that philosophy is a subject which is almost certainly of its very nature *non-scientific*. We must not jump from this purely negative statement to the conclusion that it has the positive defect of being *unscientific*. The latter term can be properly used only when a subject, which is capable of scientific treatment, is treated in a way which ignores or conflicts with the principles of scientific method.

(2) One of the adverse comments which we noted above was that

philosophy makes no progress and that opposite views on the same question simply alternate with each other again and again. I think that, at any rate in some important instances, this is a superficial appearance which is greatly modified by closer inspection and more intimate knowledge of the facts.

We must first distinguish here two different cases, viz., certain very general and far-reaching alternative types of philosophic theory, and alternative views on certain fairly specific problems. As an example of the former we might take what I call 'Substantial Monism' and 'Substantial Pluralism', with the philosophy of Spinoza and that of Leibniz as typical and extreme instances of each. As an example of the latter we might take the cluster of interconnected problems about the nature of sense-perception and its objects.

As regards alternative theories of the first kind, I think we must expect them to co-exist with each other at every period in the history of philosophy. Unity and plurality, in intimate correlation, are obvious features in the world as it presents itself to any reflective person. The aspect of unity appeals intellectually and emotionally to thinkers of one temperament, and that of plurality appeals equally strongly to those of another. Each type of theory is likely to be held, though in various specific forms, by equally intelligent philosophers at every period of history. Neither party is in the least likely to convert the other to its own view by argument. At best, argument may be used to show that a particular theory of one or other of these alternative types fails to do justice to a certain important aspect of unity or of plurality, as the case may be. Apart from this it is a waste of time to argue. One has to make one's choice of one or another of these *types* of theory on grounds which cannot be presented in the form of arguments appealing equally to all intelligent and instructed persons. Having done this, one may try to develop the chosen type of theory into as coherent and comprehensive a specific form as possible. In that undertaking there is plenty of room for argument, and there is no reason why there should not be progress.

As an example of the second case we might take the controversies about sense-perception and its objects which have so much occupied English philosophers from the time of Locke to the present day. It might well seem to an unsympathetic external observer that idealistic and realistic answers have merely alternated with each other and that no progress has been made. Now this is certainly a mistake. The course of philosophical thought here, amongst really able and expert thinkers at any rate, has taken the form of a spiral and not of a mere circle.

Any competent philosopher who puts forward either a realistic or an idealistic answer to these questions nowadays must state it in a far more subtle and qualified form than his remote predecessors would have done, just because of the distinctions which have been drawn and the criticisms which have been made by intermediate philosophers in the course of their controversies with each other. In consequence of this, his theory, of whichever type it may be, should at least be immune to many of the objections which have been legitimately brought against earlier forms of the same type of theory.

Another excellent illustration of the same fact is provided by the controversies in regard to phenomenalism and the so-called 'verification-principle' which have taken up so much of the energies of philosophers in England and the U.S.A. during the last twenty-five years. I think it might fairly be said that phenomenalism began as a new gospel, that its adherents strove with immense ingenuity and evangelical fervour to apply the verification-principle to cases which seemed *prima facie* to be most recalcitrant, and that in the end it has had to be admitted that a satisfactory phenomenalist analysis of, e.g., material-object propositions or memory propositions, cannot be given. To an unsympathetic observer it might well seem that we were simply back at where we started, and that a number of very able men, who might surely have been better occupied, had wasted a quarter of a century in desperately defending positions which any sensible person could have seen at the outset to be untenable.

I am quite sure that such a judgment is superficial and mistaken, though I must confess that I have sometimes myself felt inclined to say such things in my haste. The fact is that, in the course of the defence and the attack, ambiguities in the at first vaguely formulated verification-principle have been noted and cleared up, and much light has been thrown on the conditions which must be fulfilled by any satisfactory analysis of material-object propositions or of memory propositions.

I think that the fate of the verification-principle provides an instance in favour of a rather depressing generalization which the history of philosophy suggests. It is this. Time and again some new general principle is brought forward with a flourish of trumpets. As originally formulated it is vague and ambiguous, but this is not at first apparent. In the course of controversy the ambiguities are cleared up, and then it is found to cover two or more alternatives. On one interpretation it is certainly true, but is little more than a tautology and has no interesting consequences. On the other interpretation it is synthetic and would have

exciting consequences, but there is no reason to think it true. The time for evangelical fervour is when the ambiguity is still undetected and the principle seems to be at once evidently true and fruitful in consequences. This depressing generalization extends beyond the field of philosophy. It is admirably illustrated, e.g., by the so-called 'economic interpretation of history'. Such illusions are not to be despised. They may be compared with those which cause men to marry and beget children. They are carrots which nature dangles before the noses of philosophers, luring them on to do much valuable thinking which would otherwise never have been undertaken.

(3) The last remark which I will make here is the following. There certainly are changes of fashion in philosophy. When a new kind is in vogue, many things in the philosophy of an earlier day, which are of permanent value and perhaps highly relevant to contemporary problems, tend to be altogether forgotten or carelessly and ignorantly dismissed, simply because they occur in an out-of-date setting and are clad in an unfashionable dress. It is now quite certain that much of permanent value in Scholastic philosophy was ignored or contemned from this cause by Locke, Berkeley, Hume, and their followers. I have little doubt that the same is true *mutatis mutandis* of the attitude of many present-day philosophers towards the systems of monistic idealism which were fashionable at the beginning of this century. It is consoling to a philosopher's vanity not to pry too closely into the history of his subject, for otherwise he is liable to find that his discoveries have been anticipated and his fallacies refuted in advance by predecessors whom he has ignored or despised.

I want now to consider in some detail a view of the nature of philosophy which has been accepted in recent years by a number of very able thinkers in England and the U.S.A. It may be formulated roughly as follows. The sole or the main business of philosophy is to analyze the various kinds of proposition which constitute the common-sense view of the world. For many philosophers this line of thought originates in the paper *A Defence of Common-Sense*, which was Professor G. E. Moore's contribution to the collection of essays published in 1925 under the title *Contemporary British Philosophy*. It will be worth while to devote some careful attention to this famous and very influential essay of Moore's.

Moore does not attempt to define the word 'common-sense' or the phrase 'a common-sense proposition'. Instead he begins by enumerating a long list of propositions, each of which would, I think, readily be

admitted to be a common-sense proposition. In order to classify them briefly and intelligibly I will begin by introducing a few simple technical terms. Let us call a proposition 'epistemic' if and only if it asserts that a certain individual, or some one or other, or everyone *knows* a certain proposition or *knows* propositions of a certain class. Thus the following propositions would be epistemic: Jones knows that the angles at the base of an isosceles triangle are equal; Everyone knows that twice two is four; Some people know all the propositions in the First Book of Euclid. On the other hand, the proposition $2 \times 2 = 4$ would be non-epistemic. An epistemic proposition may be of the first or the second or of a still higher *order*. The examples which I have given are all of the *first* order. But the proposition: I know that Jones knows that $2 \times 2 = 4$, is an epistemic proposition of the *second* order.

The first point to be noted is that Moore's propositions fall into two primary groups. The first consists of a large number of *non-epistemic* propositions. The second consists of a single *epistemic* proposition of the *first order* concerning the members of the first group.

The non-epistemic propositions which constitute the first primary group may themselves be subdivided first into *physical* and *psychological* propositions. The physical propositions may then be subdivided into *autobiographical*, *heterobiographical* and *non-biographical*. The autobiographical physical propositions are assertions *by Moore himself* of the existence, and certain spatial and other relationships, of *Moore's own* body during a certain period. The heterobiographical ones are similar assertions made by Moore about other living human bodies and the same or earlier periods. The non-biographical ones are similar assertions made by Moore about certain ostensibly non-living bodies, e.g., the earth and the sun, and the same or earlier periods. Finally, the psychological non-epistemic propositions may be subdivided into a number of *autobiographical* ones and a single *heterobiographical* one. The autobiographical ones are assertions by Moore that he himself has from time to time had experiences of each of certain specific kinds. The single heterobiographical one is an assertion by Moore, with regard to the human bodies other than his own, whose existence he asserted in the heterobiographical physical propositions. It is that many of these have been the bodies of *persons* who have had during the lifetime of their bodies experiences of the various kinds which Moore himself has had during the lifetime of his present body.

So much for the first primary group, i.e., the *non-epistemic* propositions. We come now to the second primary group, which, as I have said,

consists of a single *epistemic* proposition of the *first order*. This may be put as follows. It is true of many of the persons other than Moore himself, whose existence is implied by the heterobiographical psychological proposition just stated, that they have often during the life time of their present bodies *known* propositions corresponding *mutatis mutandis* to each of those enunciated in the first primary group. The *mutanda* here are of course such terms as the person denoted by the proper name 'Moore', the date at which Moore was born, the date at which he wrote the essay, and so on, in the autobiographical propositions.

Now Moore claims to *know* many propositions in each subdivision of the first group. Since this is an assertion of knowledge, it is an *epistemic* proposition. Since all the propositions in the first group are *non-epistemic*, it is an epistemic proposition of the *first order*. But Moore also claims to *know* the one proposition in the second group. Since this is itself an *epistemic* proposition of the *first order*, the assertion that Moore knows it is an epistemic proposition of the *second order*.

We may sum up this complicated business as follows: (1) Moore claims to *know* that he has a *body* of a certain kind, viz., a living human body, and that he has had *experiences* of certain kinds. He claims to know that there are *other living human bodies* beside his, and that there are *bodies of other kinds*, e.g., trees, chairs, etc. He claims to *know*, with regard to each living human body, that it is the body of a *person* who has had experiences of the same kinds as he himself has had. [All this is an epistemic proposition of the *first order*.]

(2) Moore claims to *know*, with regard to each such person, that *that person knows* facts corresponding *mutatis mutandis* to each of the kinds of fact enumerated above which Moore himself claims to know. [This is an epistemic proposition of the *second order*.]

The next point to be noted is this. The word 'know' is used in English in such a way that it would be nonsensical to say of a proposition that it is known by someone but may yet be false. Therefore anyone who admits Moore's claims must admit that there are true propositions in all the subdivisions of Group I. He must also admit that the proposition which is the only member of Group II is true. There are two ways in which he might seek to whittle down this admission. One is to say that in each of the final subdivisions enumerated above there are propositions which are at any rate *partly* true, though every one of them is also partly false. The other is to say that for the propositions in each of the final subdivisions it is possible to find an *interpretation* in which some of them are wholly true, though in their ordinary sense they are

all false. We must note that Moore explicitly rejects both these expedients. He is committed to holding that in each of the final subdivisions there are propositions which, when taken in their *ordinary* sense, are *wholly* true.

This last point deserves further consideration. Moore asserts that each of the common-sense propositions which he has taken as examples, and which he claims to know, has *one and only one* meaning, and that everyone who knows English understands that meaning. Take, e.g., the proposition: There is a penny on the table before me now. Moore would say that this has one and the same *meaning* (allowing for the systematic ambiguity of the words 'I' and 'now') for everyone who understands English. But he says that there might be, and in fact are, great differences of opinion as to the *right analysis* of this and similar propositions, and he does not claim to know the right analysis of them. Unfortunately he does not further discuss these two vitally important notions of the *meaning* and the *correct analysis* of a proposition, nor does he explain in any detail how he supposes the two to be connected. All that he says on that point is this. It is impossible to raise the question: What is the correct analysis of such and such a proposition?, unless one understands the meaning of the proposition.

On this I would make the following comments. (1) Surely it is not propositions, but *sentences*, which can significantly be said to have a meaning. A proposition is what a sentence means, or more accurately it is the common meaning of a whole multitude of equivalent sentences, spoken or written, in English and in German, and so on. If this be so, we must substitute for Moore's original assertion some such statement as follows. Any person who knows English will think of one the same proposition whenever he sees or hears or utters or images an instance of the type-sentence 'There is a penny on the table before me now' Now this seems to me to be an empirical assertion about the behaviour of members of a certain class of men when placed in situations of a certain kind. I do not see how Moore or anyone else can be justified in affirming it with complete confidence. On general grounds it seems to me highly doubtful. The utmost that I could admit is that we could describe fairly accurately the kind of circumstances which would be *necessary*, though not sufficient, conditions for a sane waking adult Englishmen to utter or to write (as distinct from babbling or scribbling) an instance of this type-sentence. We could also describe fairly accurately the sort of circumstances which would be held to be relevant for testing the truth or falsity of such an utterance.

(3) Since a *proposition* cannot significantly be said to have a meaning, we cannot significantly contrast the meaning of a proposition with its analysis. We must substitute for this the contrast between a proposition *itself* and that set of interconnected propositions which together constitute the correct analysis of it. For Moore's assertion about meaning and correct analysis we could then substitute the following statement. Although everyone who understands English thinks of one and the same proposition whenever he sees or hears or utters or images an instance of the type-sentence 'There is a penny on the table before me now', yet there is no agreement as to the set of interconnected propositions which together constitute the correct analysis of that proposition. I have already commented adversely on the first part of this assertion. Let us, however, waive these objections and consider the second part of it.

(3) I suppose it would be admitted that in order for a conjunction of propositions $p_1 \& p_2 \& \dots p_n$ to count as the correct analysis of a proposition p the following conditions are *necessary*. (i) That if p is true, then $p_1 \& p_2 \& \dots p_n$ is true, and conversely. (ii) That this equivalence is not merely contingent, like the equivalence between being a ruminant and having cloven hoofs, but is in some sense *necessary*. (iii) That the equivalence, though necessary, is *not merely linguistic*. By this I mean that it really is a case of two different *propositions*, viz., p , on the one hand, and $p_1 \& p_2 \& \dots p_n$, on the other, and not merely a case of two different *sentences*, e.g., 'This is a negro' and 'This is a black man', which in a certain language stand for *one and the same* proposition.

Now, although these conditions are severally necessary for a set of propositions to count as the correct analysis of a given proposition, I think it is certain that they are not jointly sufficient. Some further condition seems to be needed, and I do not know what it is. I think that this can be made plain by the following example.

Take the sentence ' n is a prime number'. Here I think it is probably true that practically everyone who understands English and has learned elementary mathematics does attach one and the same meaning to it, viz., that n is an integer which is not exactly divisible by any other integer. Now consider the following sentence: — 'The immediate successor of the product of all the integers less than n is divisible by n '. I think it is plain that this really does mean a different proposition from that which is meant by the sentence ' n is a prime number'. So our third condition is fulfilled.' Now it can be proved that the proposition which is meant by this sentence entails and is entailed by the proposition which

is meant by the sentence ' n is a prime number'. (This mutual entailment is known as Wilson's Theorem.) So the first and second of our conditions are also fulfilled. But, although all three conditions are thus fulfilled, would anyone be prepared to say that this complicated proposition is the 'correct analysis' of the proposition that n is a prime number? I do not think that anyone would.

If anyone did, he might be invited to consider the following questions. There are plenty of other complicated propositions which can be shown to entail and be entailed by the proposition that n is a prime number. Are *all* of them to be counted as correct analyses of the proposition that n is a prime number? If so, the phrase '*the* correct analysis' has no application. If, on the other hand, one of them is to be singled out as *the* correct analysis, whilst the rest are to count only as logical equivalents and not as analyses, on what principle is the distinction to be drawn?

The only suggestion that I can make here is the following. It might be said that the proposition that n is a prime number does not *by itself* entail the proposition that the immediate successor of the product of all the integers less than n is divisible by n . The latter proposition is entailed only by the *conjunction* of the former with the axioms of arithmetic. We are inclined to overlook this because these axioms are themselves necessary propositions which are the common premisses of all deductions within pure arithmetic. Perhaps, then, we ought to add the following as a fourth condition which is necessary if a conjunction of propositions $p_1 \& p_2 \& \dots p_n$ is to count as an *analysis* of a given proposition p . The proposition p must *suffice by itself* to entail the conjunctive proposition $p_1 \& p_2 \& \dots p_n$, and similarly the latter must suffice by itself to entail the former. Whether these four conditions are jointly sufficient as well as severally necessary to mark out an analysis, I do not know. Nor do I feel any confidence that the fourth of them is compatible with the third, viz., the condition that analyzandum and analysis shall be two *propositions* and not just two different *sentences* which in a certain language mean the same proposition.

(4) I think that I can sometimes say with confidence that a certain set of propositions is *not* an analysis at all (and therefore not the correct analysis) of what I understand by a certain sentence. But I cannot state any satisfactory criterion by which I decide this. Still less can I state any criterion by which I could recognize that a certain set of propositions *is* the correct analysis of what I understand by a certain sentence when that is in fact the case. In view of all this, I do not think that it is particu-

larly illuminating to say that what is *meant* by such a sentence as 'There is a penny on the table before me now', when understood in its ordinary sense, is and is known to be wholly true, and that the business of philosophy is to seek for the *correct analysis* of such propositions and not to question their truth.

It should be noted that Moore himself never alleged that this was the *only* business of philosophy. Still less did he claim that the philosophical analyses of common-sense propositions must themselves be expressed in the language of common-sense. Some philosophers in recent years seem to me to have written as if they held the latter view. If anyone really does hold it, I can only say that it seems to me completely unjustifiable. The philosophical analysis of common-sense propositions, whatever it may really consist in, is obviously a reflective activity carried out by certain specialists upon materials provided by common-sense. It is no part of the business of common-sense itself. There is therefore not the least reason to expect that the language of common-sense would contain a suitable terminology for expressing the results of philosophical reflexion even on nothing but common-sense propositions. So it is no *prima facie* objection to a proposed philosophical analysis that it is expressed in technical terms, e.g., 'sense-datum', 'appearance', 'pure ego', etc., which either do not occur at all in common-sense language or occur there in a different and non-technical sense. One might as well object to a theoretical physicist because he uses the word 'entropy', to which no word in everyday speech corresponds, or because he uses the word 'energy' in a specialized technical sense which is very different from that which it has in daily life.

Certainly it is most desirable to bring high-sounding technical terminology to the test of concrete situations, as Professor Moore has so often done with such devastating effect. It is also true and important to note that a technical term may and often does presuppose a certain theory, and that those who habitually use it are therefore in danger of unwittingly assuming that theory without question, when it is in fact open to doubts and objections. But, provided that a philosopher always bears these facts in mind, he need not reproach himself if he sometimes expresses the results of his reflexions in terms which would be unintelligible to his bedmaker or unfamiliar to his bookmaker. It is as well to remember that the word 'jargon', which is sometimes bandied about in this connexion, is a question-begging epithet. In practice it generally denotes simply the technical terminology favoured by one's opponents.

I will conclude to-day's lecture by summarizing my own reaction to the view that philosophy is or should be the analysis of common-sense propositions.

I think that *one* very important part of philosophy *is* something which it is convenient to call the 'analysis' of various important types of proposition, though I must confess that I cannot give a satisfactory definition of 'philosophical analysis' or a criterion for deciding whether a certain set of propositions is or is not the correct analysis of what is meant by a certain sentence. I also think that *one* very important class of propositions to be subjected to philosophical analysis are those which plain men and philosophers alike express by the sentences which they utter, and understand by the sentences which they hear, in the most ordinary business of their everyday life.

I would point out however that, in my opinion, the importance of these latter propositions, and therefore the importance of a correct analysis of them, would be very little diminished if I rejected, as I am inclined to do, Moore's claims to knowledge here. Suppose I were content to say that in my unreflective moments I *unhesitatingly believe* such non-epistemic propositions as Moore claims that he and I *know*, including propositions that there are other living human bodies and that many of them are the bodies of persons who have experiences of similar kinds to those which I *unhesitatingly believe* myself to have had. Suppose, further, I were content to say that in my unreflective moments I *unhesitatingly believe* that these other persons, whom I unhesitatingly believe to exist and to have experiences like mine, *unhesitatingly believe* in *their* unreflecting moments propositions similar *mutatis mutandis* to those which I unhesitatingly believe in *my* unreflective moments. Then the importance to *me* of these propositions, and therefore of a correct analysis of them, would still be very great. And presumably the importance of similar propositions *mutatis mutandis* to another person would be no less great, if, as I unhesitatingly believe in my unreflective moments, there are other persons like myself.

However that may be, I do *not* think that the analysis of propositions of various important kinds is the *whole* business of philosophy. Nor do I think that the propositions of common-sense, as exemplified by Moore in his list, are the *only* suitable materials for philosophical analysis. It seems to me that another important part of the business of philosophy is to consider the *consistency* of common-sense propositions of various kinds with each other and with propositions of other important kinds, e.g., those of mathematics, of natural science, of morality, of psychical

research, and of religion. It is also part of its business to consider the internal consistency of each of these systems of propositions.

Both for this reason, and for its own sake, it is part of the business of philosophy to try to analyze, e.g., scientific propositions and moral propositions, as well as common-sense propositions. Here we shall have to extend the phrase 'analysis of propositions' to cover the attempt to show that certain kinds of sentences in the indicative, e.g., moral ones such as 'Lying is wrong' and 'People ought to pay their debts', do not in fact express or convey propositions at all. This extension can easily be made by substituting for the phrase 'analysis of propositions' the phrase 'analysis of what is expressed by sentences in the indicative'.

In this lecture I have confined myself in the main to negative and critical statements about philosophy itself and about certain views as to its nature and functions. In the next lecture I shall try to develop and to illustrate the ideas which I have thrown out in the course of my criticism of the view that the sole function of philosophy is to analyze common-sense propositions.

PHILOSOPHY (II)

In the present lecture I shall be covering in part the same ground as in the address, entitled *Some Methods of Speculative Philosophy*, which I gave at the joint session of the Mind Association and the Aristotelian Society at Cambridge in July 1947. As I have not substantially altered my views since then, it is inevitable that I should to some extent repeat my own words. For this I must apologize to those who happen to have read and to remember that address.

We may begin with the platitude that philosophy is what philosophers do. If, then, we want to decide what philosophy is, we shall naturally begin by considering what kind of activities have been pursued by men whom everyone would regard as great philosophers when engaged in what everyone would regard as their characteristically professional work. There would be no great difficulty in giving instances which would satisfy most people. Spinoza and Leibniz were great philosophers; Shakespeare and Gibbon, though men of the highest intellectual calibre, were not philosophers at all. Spinoza was engaged in philosophical work when he wrote his *Ethics* and not when he wrote his *Hebrew Grammar*, and

Leibniz was so engaged when he wrote his *Monadology* and not when he wrote his history of the House of Brunswick. But there are undoubtedly plenty of marginal cases. One and the same man may be eminent both as a philosopher and as a scientist or a mathematician. Descartes, Leibniz, and Whitehead are obvious examples. Then, again, it may be impossible to draw a sharp line between philosophical and non-philosophical activities in many cases. Einstein, e.g., was obviously a great mathematical physicist and not primarily a philosopher, but it might well be held that the reflexions which led him to formulate first the special and then the general theory of relativity were predominantly philosophical. Leibniz's writings on dynamics and on the differential calculus seem to be an intimate blend of what everyone would call 'science' and what most people would call 'philosophy'.

We may notice next that what would generally be admitted to be philosophical activities seem to cover at least two very different kinds of intellectual undertaking. Hume's attempt to analyze causal propositions and Spinoza's attempt to establish by deductive argument the fundamental nature of God or the Universe are extreme instances of the two. In the philosophical work of most great philosophers these two kinds of activity are blended in various proportions. But it is not at all obvious that there is any necessary connexion between them. It might be held that the former is a practicable and useful activity, which should continue to be pursued; whilst the latter is an impracticable but seductive activity, which should be dropped, and against which the unwary should be warned. (Those who take this view might claim to find analogies in the original amalgam of astronomy with astrology and of chemistry with alchemy.)

Now I am inclined to think that there is one feature which is characteristic of all work that would generally be regarded as philosophical. This I will call 'Synopsis'. I am inclined to think that there are certain other features, of which the following things may be said. One or more of them must be present, in addition to synopsis, in any work which would be called 'philosophic'. But they need not all be present. In some work which everyone would call 'philosophic' one or more of these features may be absent or evanescent. I distinguish four such features, and I will give to them the following names, 'Analysis of Propositions and Concepts', 'Detection and Formulation of Presuppositions', 'Critical Appraisal of Presuppositions', and 'Synthesis'. The first three of these are very closely bound up with each other, and together they are characteristic of what I call 'critical philosophy'. The fourth, viz., synthesis, is

specially characteristic of what I call 'speculative philosophy'. The rest of this lecture will be devoted to explaining and illustrating the above statements and pointing out inter-connexions between these various features. I will begin with *synopsis*, which I regard as fundamental.

There are different departments of fact, or different regions or levels within a single department, which are very seldom viewed together or seen in their mutual relationships by the plain man or even by the professional scientist or scholar. Yet they do co-exist and are relevant to each other and must presumably be inter-connected into some kind of coherent whole. All men at most times, and many men at all times, conduct various parts of their living and thinking in relatively watertight compartments. They turn blind eyes to awkward, abnormal, or marginal facts, and they skate over the surface of phenomena. A strong and persistent desire to see how the various aspects of experience hang together is perhaps the one characteristic common and peculiar to philosophers. I understand by the word 'synopsis' here the deliberate attempt to view together aspects of human experience which are generally viewed apart, and the endeavour to see how they are inter-connected. To illustrate what I have in mind I will take two of the examples which I used in my address of 1947.

As our first example we will take the problem of sense-perception. We commonly think of a material thing as something which has all the following characteristics. It is something which has at every moment a certain shape, size, and position in a common three-dimensional space; which has at every moment various qualities, such as colour and temperature, in certain determinate forms; which has various causal properties, such as inertia, impenetrability, elasticity, etc.; and which persists, moves or rests, retains or alters its determinate shape, size, qualities, and causal properties, and interacts with other material things through impact, gravitation, radiation, etc. We commonly think that our senses reveal to us the presence and certain of the qualities, mutual relations, changes, and causal properties of certain bodies. We assume that these bodies would have existed; would have had precisely the same qualities, mutual relations, and causal properties; and would have undergone precisely the same changes, even if they had never been perceived by anyone, human or non-human. We take for granted that at the time when a person sees a certain object, e.g., a star, that object exists and is in the state and at the place in which it then visually appears to him to be. We think that the same person can perceive the same part of the same body at the same time by different senses, e.g., sight and touch; and that different persons

can perceive the same part of the same body at the same time by the same sense, e.g., sight.

Now there is a problem about sense-perception for the following reasons. (1) If we attend carefully, we note such facts as these. (i) Of two persons, who would be said to be seeing the same part of the same thing at the same time, one may see it, e.g., as round and the other as elliptical. (ii) One and the same person, who would be said to be seeing and feeling the same part of the same thing at the same time, may, e.g., see it as elliptical and feel it as round. (iii) One and the same person, who would be said to have been seeing the same *unchanged* part of the same thing at different times from different positions, may, e.g., see it on one occasion as round and on the other as elliptical. Common-sense is more or less aware of such minor systematic variations in normal sensible appearances, and it has certain modes of expression for describing them, but in the main it ignores them. Certain sciences and arts, e.g., geometrical optics and perspective drawing, deal explicitly with some of them.

(2) There are visual perceptual experiences which are abnormal in various ways and degrees, but are similar to and continuous with normal visual perceptions. They range, e.g., from mirror-images and sticks which feel straight but look bent when half immersed in water, through 'seeing double' when one eyeball is pressed aside or the percipient is drunk, to dreams and full-blown waking hallucinations. Now those which come at the latter end of this scale cannot plausibly be interpreted in the naively realistic way which ordinary language inevitably suggests for normal sense-perceptions, and yet qualitatively the series is continuous from one end to the other.

(3) There are certain highly relevant facts which are still quite unknown except to a minority of grown-up and educated persons. These must have been completely hidden from everyone at the time when the language in which we express our perceptual experiences was first formed and for thousands of years afterwards. One of these is the *physical* fact that light takes time to travel, and that a visual experience referring to an event in a remote object does not begin until light which left that object simultaneously with that event reaches the percipient's eye. A consequence is that an experience which would naturally be described as 'seeing a certain remote object at a certain moment' does not guarantee the existence of any such object at *that* moment, nor does it reveal the *contemporary* shape, size, position, colour, etc., of such an object if it does still exist. At the best such an experience guarantees

only the existence of such an object in the more or less remote *past*, and reveals only the shape, size, position, colour, etc., which it *then* had. To this physical fact we must add the *physiological* fact that visual appearances vary with certain differences in the percipient's eye, optic nerve, and brain, even when the retinal stimulus is the same. We must also add the *psychological* fact that visual perceptions are determined to some extent by the percipient's past experiences and present mental attitude and expectations.

Now there is a philosophical problem of sense-perception for those and only for those who try to envisage all these facts together and to give an account of sense-perception and its objects which fits them all into a coherent pattern. The language in which we express our sense-perceptions and talk about material things was formed unwittingly in pre-historic times to deal in a practical way with a kind of normalized extract from our total perceptual experience. It was formed in utter ignorance of a whole department of highly relevant physical, physiological, and psychological facts. It would surely be nothing short of a miracle if it were theoretically adequate and if it were not positively misleading in some of its implications. If a synoptic view is to be taken, it must be taken by persons with philosophic interests and training, who are adequately informed of these and other relevant facts beside the beliefs and linguistic usages of common-sense.

As my second example I will take what may roughly be called the 'mind-body' problem. Here the main facts to be viewed synoptically are the following.

(1) Everyone knows that many of his sensations and feelings follow closely upon, and vary concomitantly with, certain events in his eyes, ears, skin, joints, etc. On the other hand, many experiences, e.g., those of day-dreaming, reasoning, deliberating, etc., do not seem *prima facie* to be co-variant with events in one's body. Then, again, everyone knows that certain of his overt bodily movements follow closely upon, e.g., his intentions to express certain thoughts or to make certain changes in his own or foreign bodies. There is no doubt at all that the common-sense notions of cause and effect, and the associated ideas of agent, instrument, and patient, and so on, are mainly derived from such facts as these.

(2) The sciences of anatomy and physiology make it almost certain that the *immediate* bodily antecedents and correlates of our sensations are *not* events in our eyes, ears, skin, etc., but are slightly *later* chemical or electrical changes in our brains. These can be made perceptible to the senses only indirectly through elaborate technical devices, such as the

electro-encephalograph. These sciences also make it almost certain that the *immediate* bodily consequents and correlates of setting oneself to fulfil an intention are *not* the overt bodily movements which one is setting oneself to make, but are slightly *earlier* chemical or electrical changes in one's brain. Of these one is completely ignorant. It is further alleged, on the authority of these sciences, that there are immediate cerebral antecedents and correlates, of the same general nature, even for those mental processes, such as day-dreaming, reasoning, deliberating, etc., which do not seem *prima facie* to be co-variant with events in the body.

(3) The physical sciences have developed a concept of causation in terms of regular sequence and concomitant variation. In this the notions of agent and patient, activity and passivity, etc., play little if any explicit part. So far as the notion of acting and being acted upon survives in physics it comes to this. The physicist thinks of one system as acting on another when energy is transferred from the former to the latter, or when the former, without doing work on the latter, modifies the direction of motion of some of its parts by fixed constraints. Now there is fairly good empirical evidence that a living organism never gains or loses energy except by transference from or to some other part of the *material* world. And it is difficult to imagine a volition exercising guidance without work on moving atoms or electrons in the brain, as a material constraint, such as the rod of a pendulum, does on a moving macroscopic body, such as a pendulum-bob.

Now these various mutually relevant facts and concepts and principles are hardly ever viewed synoptically, except by philosophers. Common-sense is quite ignorant of many of them, and common language has grown up and crystallized before they were known or suspected. On the other hand, scientists, though familiar with all of them, tend to concentrate on one at a time and temporarily to ignore the rest. When they confine their attention to the chemical, physical, and physiological facts they are inclined to take the view that men are 'conscious automata', i.e., that all our mental states, including processes of deliberating, imagining, reasoning, etc., are mere by-products of states of the brain, which are themselves completely determined by physical and physiological antecedents. But their daily lives and all their professional activities in designing, carrying out, and interpreting experiments presuppose a view which is shared by plain men and which seems *prima facie* to be incompatible with the 'conscious-automaton' theory.

A scientist always assumes in practice that, when he designs an experiment and carries it out, he is initiating certain changes in the material

world which would not have taken place then and there unless he had thought them out beforehand, willed them, and deliberately prepared the conditions for them. He assumes that his assent to or dissent from the various alternative explanations which might be put on the results of an experiment is determined by processes of *reasoning*, demonstrative or probable, in which assent is given or withheld in accordance with *evidence*, which may be favourable or unfavourable, weak or strong or coercive. Now all this *certainly* involves concepts, and seems *prima facie* to involve modes of causation, completely different from those in terms of which the 'conscious-automaton' theory is formulated.

To sum this up briefly. A scientist who investigates and theorizes about man and his powers and activities is himself a man exercising certain characteristically human powers and activities. But the account which he is led to give of man, when he treats him as an object of scientific investigation, seems difficult to reconcile with the occurrence and with the validity of his own most characteristic activities as investigator, experimenter, and reasoner. It seems no less difficult to reconcile with what non-scientists, and the scientist himself in his daily life, unhesitatingly take for granted about themselves and their fellow-men in ordinary social intercourse. There is obviously need for synopsis by someone who is aware of all the main facts and can hold them steadily together in one view. It is for that reason that there is a philosophical problem of body-and-mind, even if the solution of it should consist in showing that it has been wrongly stated.

I think that these two examples should suffice to show what I understand by 'synopsis' and why I think that it is a mental operation which is peculiarly characteristic of philosophy. Synopsis is, however, no more than an essential first stage. From it philosophical thinking may develop in various ways. I will now try to describe and illustrate these.

Suppose that the results of taking a synoptic view of a number of different mutually relevant departments of knowledge or belief were to show that they all obviously fit together without difficulty into a single coherent whole. Then there would be little or no occasion there for philosophy. But actually, as illustrated by my examples, this is often not the case. It often happens that each of the several mutually relevant regions, which we habitually contemplate and react to separately, gives rise to its own system of concepts and principles; that each such set seems fairly satisfactory and coherent in isolation; but that, when we contemplate the various departments together, we find that the several sets of concepts and principles seem *prima facie* to conflict with each

other. It is synopsis, revealing *prima facie* incoherence, which is the main motive to philosophical activity.

When we are faced with apparent conflict between different sets of propositions which, taken separately, seem to be satisfactory and evidently true, the most obvious first step is to try to analyze the terms in them and to formulate the propositions themselves more carefully. Such a process is an indispensable step towards deciding whether the inconsistency is real or only apparent, and towards formulating it precisely if it is real. And this is a precondition of any reasonable attempt to deal with the difficulty.

Now the general principles, in accordance with which people do in fact think and behave in their everyday life, in their professional activities as scientists, in their religious life, and so on, are seldom if ever explicitly before their minds. They have never explicitly formulated these presuppositions for themselves, and they might not recognize them if another person were to do so for them. It is an important part of philosophic activity to try to elicit and formulate the presuppositions of each of the various important departments of knowledge and belief which are to be brought into one synoptic view. This is obviously a difficult and delicate task. It is particularly so in connexion with the presuppositions of common-sense. For here the philosopher is trying to do for the plain man what the latter could not attempt to do for himself without ceasing, by definition, to be plain. It is evident that there is a serious risk of putting into the mouths of babes and of sucklings what could have occurred only to the wise and prudent. Nevertheless this danger must, I think, be faced if we are to get anywhere.

I will now give some examples of attempts to elicit and formulate presuppositions. I have already outlined one instance in the account of the common-sense view of material things and our perception of them with which I introduced my first example of synopsis. Another admirable example is Sidgwick's attempt in his *Methods of Ethics* to formulate the main principles of the morality of common-sense. He tries there to elicit and to state clearly the presuppositions at the back of everyday moral judgments about truth-telling, promise-keeping, justice, etc., and their opposites lying, promise-breaking, injustice, etc. This seems to me a typically philosophical undertaking, performed with exemplary acuteness, fairness, and thoroughness.

It is often far from easy to discover what is presupposed by a certain system of beliefs or behaviour. What one tries to do may be described very roughly as follows. One tries to formulate a minimal set of fairly

general propositions which would make it logically necessary or logically reasonable for a person who accepted them, and who thought and acted consistently, to believe and to act in particular relevant situations in the ways in question. Here it is important to remember that we cannot safely assume that people do think and act consistently. We know that each of us is occasionally inconsistent in detail, and it would not be surprising if all of us were always more or less inconsistent in principle in important departments of our belief and behaviour. It may therefore be impossible, from the nature of the case, to formulate a mutually consistent set of presuppositions even for a particular department of human thought and action, e.g., for common-sense morality or for the common-sense beliefs about sense-perception and material things. Sidgwick, at any rate, found it impossible to do so for common-sense morality. If the presuppositions of any important department of belief or action seem to be mutually inconsistent, or if those of one department seem to be inconsistent with those of another which is obviously closely connected with the former, then the need for a careful analysis of the propositions concerned becomes pressing.

We can illustrate this also from the problem of sense-perception. In order to deal with *prima facie* conflicts and difficulties here we need to analyze carefully such notions as the following: the notion of perceiving the same part of the same thing by two different senses, e.g., sight and touch; the notion of two persons simultaneously seeing the same part of the same thing; the notion of a thing remaining objectively unchanged during a period in which one and the same part of it presents varying visual appearances to an observer; the notion of a perceived thing having parts, e.g., a back and an inside, which are not at the moment being seen or felt; and the notion of a perceptible thing existing and having a history during periods when no one is perceiving it.

Another good example is provided by the problem of free-will and determinism. The situation here may be described roughly as follows. (1) There is a sense of 'could' in which each of us in his daily life is quite convinced that on many occasions he and others *could* have acted otherwise than they in fact did. (2) Unless this conviction is significant and true, *all* judgments of the form: You *ought* to have done *X* (which you did *not* do.), or: You *ought* not to have done *Y* (which you *did* do.), are *in principle* false. Moreover, if that be so, such morally directed emotions as remorse, moral indignation, etc., are and have always been altogether without appropriate objects. (3) On the other hand, it seems evident to many people on reflexion that, given the circumstances in

which any event happened and the laws of nature, including those of psychology, it is *inevitable* that precisely such an event should have happened then and there. In science and a large part of our daily life we do seem to presuppose this.

Now, in view of this *prima facie* conflict of presuppositions, the first business of the philosopher is one of analysis. He must try to analyze the sense of 'could' in which each of us is convinced that he and others *could* on many occasions have done otherwise than they did. He must try to analyze the sense of 'could' in which the conviction that persons could have acted otherwise than they did seems to be presupposed by a whole class of ordinary moral judgments and morally directed emotions. He must ask whether these are the same or different. Then he must try to analyze the sense of 'could' in which it seems evident to many that, given the circumstances in which an event happened and the laws of nature, nothing else *could* have happened. He may find that the senses are different, and that 'could' in the former sense or senses does not conflict with 'could not' in the latter. If so, the problem vanishes. If not, we have at least taken the essential first steps towards any possible solution of it.

I have now explained and illustrated what I mean by 'synopsis', 'detection and formulation of presuppositions', and 'analysis of concepts and propositions'. We can next consider what I have called 'critical appraisal of presuppositions'.

It is one problem, and often a very difficult one, to elicit and formulate the *de facto* presuppositions of a certain department of human belief or conduct. It is another problem to analyze these presuppositions and the notions involved in them, though these two processes generally go on together and interact with each other. But, when all this is done to the best of our ability, the question remains: Is there any good reason for us as critical philosophers to accept these analyzed and formulated presuppositions, even if as plain men or as scientists or as religious men we cannot help continuing to believe and to act in accordance with them? This is the question of critical appraisal.

Suppose that the presuppositions of a certain department conflict with each other, as Sidgwick thought that those of common-sense morality do. Or suppose that those of one sphere of thought and action conflict with those of another intersecting sphere, as, e.g., it might well be held that the presuppositions of common-sense morality and everyday social intercourse do with those of biology, physiology, and experimental psychology. Then it is plain that a rational reflective person cannot continue in theory

to accept either of the conflicting propositions without reservation. But, even when there is no apparent conflict after careful analysis has been made, a situation may arise which I will illustrate by comparing and contrasting the cases of deductive and inductive logic.

Consider the rules of the syllogism as formulated by Aristotle and his successors. There is a considerable class of deductive arguments which can without great violence be reduced to syllogistic form. Those of them which would universally be admitted to be cogent break none of the traditional rules of the syllogism, whilst those which would universally be admitted to be invalid break one or more of those rules. We may say therefore that Aristotle and his successors formulated the presuppositions of valid reasoning for an important class of deductive arguments.

Now these rules do not appear to conflict with each other or with the presuppositions of any other department of human activity. So far, so good. But it must be admitted that few if any of them have any trace of self-evidence. Is it in the least obvious, e.g., that a syllogism with two negative premisses cannot be valid; or that, if the conclusion of a valid syllogism is negative, then one of the premisses must be negative? Surely not. But in this case we can get behind the traditional rules to more ultimate principles which entail them and which are self-evident. This can be done in various ways which we need not consider in detail. So here the situation may be regarded as satisfactory.

Now contrast this with the case of inductive reasoning and the endeavours which have been made by Bacon, Mill, Venn, Jevons, Johnson, Keynes, and others to formulate and justify its principles. We need not concern ourselves here with the *eliminative* methods elaborated by Bacon, Mill, and Johnson. For, although these raise many interesting and subtle questions of detail, as Professor von Wright has well shown, they are essentially *deductive* arguments of a perfectly familiar kind. They state the conditions under which we can *reject* as false certain proposed generalizations. They give us no *direct* information as to the conditions under which we may *accept* a proposed generalization. Nor do they justify us even indirectly in accepting any generalization unless the following conditions are fulfilled. We must already know somehow that one or another of a certain set of alternative generalizations is true, and we must have empirical evidence which enables us to reject all but one of them in accordance with these principles of elimination.

It is plain, then, that the essential problem is that of inductive generalization. In its simplest form the question may be stated as follows. Suppose that a number of instances of a class α , e.g., swans, which is in

principle unlimited in extent, have been observed, and that they have all been found to have a certain property P , e.g., whiteness. What justification is there for concluding, either with certainty or with high probability, that every past instance of α had P , that every present instance of it has P , and that every future instance of it will have P ? We all assume that many such arguments are valid, and the whole of natural science is built on that assumption. What is at the back of this conviction?

Now one thing is evident on a moment's reflexion. The subject of the instantial premiss is *some*, but *not* all, members of α , viz., those which have been observed up to date. But the subject of the inductive generalization is *all* members of α . The predicate is the same in both cases, viz., the property P which all the observed instances were found to have. It is therefore clear that any attempt to regard the argument as *demonstrative* and leading to a conclusion which is *certain* must be mistaken. It does not matter what one supposes the suppressed premiss to be. *No* premiss, however strong, added to a particular premiss, will enable one validly to deduce a universal conclusion with the same subject and predicate. If such arguments can be defended at all, then, the line taken must be that the instantial premiss, either by itself or in conjunction with some other concealed premiss, makes the inductive conclusion *highly probable*.

It follows at once that, if inductive generalization can be justified at all, some at least of its presuppositions must be certain formal principles of the logic of probability. These may perhaps be regarded as self-evident, like the ultimate principles on which the traditional rules of the syllogism rest. We must remember, however, that the correct analysis of probability-propositions is a matter of acute controversy. It might well be held that the only senses of 'probability' in which these formal principles of probability are self-evident are quite different from the sense in which the conclusion of a well-established inductive generalization is highly probable.

Let us, however, waive this difficulty. Even so, it is generally admitted by experts that the utmost that could be proved is the following. In accordance with the formal principles of probability all that favourable evidence can do is to multiply the *initial probability* of a generalization. If and only if this initial probability exceeds a pre-assigned fraction, which may itself be as small as you please, then an indefinitely long repetition of exclusively favourable instances will raise the final probability as near as you please to unity.

Now, if this analysis be accepted, an essential presupposition of induc-

tive generalization must be a proposition to the following effect. For any conceivable inductive generalization there is some fraction ϵ , such that the probability of this generalization, *prior to* the observation of any relevant instances, favourable or unfavourable, is greater than ϵ . Now, *if* this is an intelligible statement, I do not think that it conflicts with anything that we positively know or with the presuppositions of any other department of thought or action. But one might well ask whether it is even *intelligible*. What exactly are we to understand, e.g., by the probability, prior to any observations on the colour of swans, of the generalization that all swans are white?

But, even if the sentence be intelligible, is there the faintest reason to accept the proposition which it states? Obviously it has no trace of *self-evidence*, and equally obviously it would be circular to offer *inductive* grounds for it. In its lack of self-evidence it is indeed no worse off than some of the traditional rules of the syllogism. But in the present case no one has been able to suggest any proposition or set of propositions which fulfil the two essential conditions of being self-evident and entailing this proposition. All suggestions that I know of, e.g., the *Uniformity of Nature* of J. S. Mill or the *Principle of Limited Variety* of Lord Keynes, break down on both tests. They are not in the least self-evident, and they are far too abstract to entail anything so determinate as the proposition in question.

The fate of this attempt to elicit, formulate, and critically appraise the presuppositions of a very important department of human thought is typical of what may happen. When one tries to appraise critically the presuppositions which one has elicited and formulated one may find that, although they do not conflict with each other or with those of any other department of thought or action, yet there appears to be no reason, direct or indirect, for accepting them. They are not self-evident, and one cannot discover any set of self-evident propositions from which they follow.

The last topic which I shall treat is the activity which I have called 'synthesis' and which I said was specially characteristic of speculative philosophy.

The purpose of synthesis is to supply a set of concepts and principles which shall cover satisfactorily all the various regions which are being viewed synoptically. The concepts and principles characteristic of each separate region, in so far as they are valid, must be shown to follow from, or at least to cohere closely with, this more general set under the special conditions and limitations peculiar to that region. The apparent

conflict between the concepts and principles characteristic of different but overlapping spheres of thought or action must be shown to arise from the valid application of these common concepts and principles in different contexts and under different special limitations. Even when there is no conflict to be solved it is likely that the synoptic contemplation of several regions, which are usually contemplated and reacted to separately, will reveal certain analogies between their contents or their structure and certain inter-relations between them as collective wholes.

As an example of philosophic synthesis I will return to Sidgwick's *Methods of Ethics*. I have already given his account of the presuppositions of common-sense morality as a good example of the processes of eliciting, formulating, and critically appraising the presuppositions of an important department of human thought and action. I will now take his Utilitarianism as an example of an attempt at synthesis.

It appeared to Sidgwick that the presuppositions of common-sense morality are neither severally self-evident nor collectively consistent. On the other hand the following propositions did seem self-evident to him. (1) When several mutually exclusive alternative courses of action are open to a person in a given situation he will do *rightly* if and only if he enacts one of them which will have *at least as good consequences* as any of the others would have if he were to enact it. (2) The only things that can be *intrinsically good or bad* are *actual experiences*, and the only feature of an experience which makes it intrinsically good or bad is its *pleasantness* or *unpleasantness*, respectively. (3) Two alternative states of affairs, in which the same net balance of pleasant over unpleasant experience is distributed among wholly or partly different individuals or is differently distributed among the same individuals, have precisely the same *intrinsic* value. These three principles may be called respectively the *Utilitarian Principle*, *Ethical Hedonism*, and the *Indifference Principle*.

Now none of these principles seems in the least evident to most people at first sight. So Sidgwick tries to point out and remove a number of confusions and irrelevancies which tend to make people think that they are contemplating *these* propositions when they are in fact contemplating, and quite rightly doubting or rejecting, certain others which they confuse with these. His hope is that, when they really contemplate the propositions which he has in mind, they will find them as self-evident as he does. This is evidently the only possible method of procedure in such cases.

Suppose now that we take into account the actual psychological and sociological facts about men living in communities. One important fact

of this kind is the limitation of men's natural sympathies, of their knowledge of others, and of their power of affecting others for good or ill. Another such fact is the importance for human welfare of having general rules of conduct in certain frequently recurring situations, which men can be relied upon to follow, without pausing to reflect on the utilitarian reasons for doing so, even when following them goes much against the grain. Sidgwick then tries to show in detail the following two things. He tries to show that, from his three self-evident principles and these psychological and sociological facts about human nature, the rules of common-sense morality follow as generally reliable *recipes* for maximizing happiness and minimizing unhappiness in a number of important types of situation in which men are frequently placed. He also tries to show that his three principles, together with these facts about human nature, provide a reasonable basis for deciding what a person ought to do in those marginal cases where the rules of common-sense morality conflict with each other. He thinks that common-sense is itself inclined on the whole to appeal to the utilitarian principle when faced with such situations. I am not called upon here to express any opinion on the success or failure of this attempt of Sidgwick's. What I do say is that it provides a most excellent example of an attempt at philosophic synthesis, based upon synopsis and subsequent formulation, analysis, and critical appraisal of presuppositions.

This example relates, however, to synthesis *within* a single region, and not to synthesis of a number of overlapping and mutually relevant regions, such as I had in view when I gave as examples of synopsis the mind-body problem and the problem of freedom and determinism. It is attempts to make a synthesis which shall cover a number of such regions, or which shall even be all-embracing, that are characteristic of the great speculative philosophers, such as Plotinus, Leibniz, Spinoza, Hegel, Whitehead, McTaggart, and Alexander. I have left the mention of such forms of philosophizing to the extreme end of my lecture because I have recently said all that I have to say about them in the address on *Some Methods of Speculative Philosophy* which I referred to at the beginning.

All that I will say here in conclusion is this. To many people these are the most typical and the most exciting products of philosophical activity. But they are also the ones for which it is most difficult at the present day to put up a convincing defence, if they are to be regarded, as their authors would wish them to be, not as poetry or mythology addressed to our emotions, but as speculations about the nature of

things, to be accepted or rejected after critical examination by our intellects. For this reason I have preferred to dwell upon those philosophical activities which, as I have tried to show by examples, probably can be pursued with some hope of genuine, if neither continuous nor spectacular, progress. I suspect that synopsis, analysis, the formulation and appraisal of presuppositions, and limited attempts at synthesis, will suffice to keep philosophers innocently, and perhaps even usefully, occupied for as long as the social conditions are likely to last which make philosophizing economically and politically possible.

THE THEORETICAL RELATIONS OF THOUGHT AND ACTION*

by

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Owing to the combined influence of marxism and pragmatism, of existentialist as well as of scientistic trends — which claim to be anti-metaphysical —, contemporary philosophy, when it examines the theoretical relations of thought and action, gives without hesitation the superiority to action, which becomes the criterion of the value of thought. Whether it is a matter of judging the value of theory by practice or the value of ideas by their consequences, whether the concrete is said to be superior to the abstract or politics to metaphysics, whether one assimilates the truth of a proposition to its possibilities of verification, in all those cases, the success of a certain action is what ultimately makes it possible for different minds to reach agreement about the validity of a thought. Critical thinking to-day gives little credit to ideas; the latter must pay cash, under the guise of immediately observable effects, for all support which they endeavour to gain. In Aristotelian terms this would be expressed as the superiority of the act over essence, and it would even go so far as to reduce the essence to the acts which are its manifestations.

It is hardly necessary to observe that this actualistic tendency, so widespread in our age, is completely opposed to the classical tradition of Western metaphysics, which in looking for first principles and necessary truths, has, since Plato and Aristotle, seen in rational thought the guide to all action and the basis of all wisdom. The classical tradition does not hesitate to assert the superiority of the eternal over what is subject to time, of contemplation and science of the immutable over practice, production and practical learning, knowledge of what is convenient. Let us remember that for many philosophies which belong to this

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tradition, — mentioning especially Stoicism and Spinozism, — action is not opposed to thought, but to passion. What matters is not pointless movement, but free action in which one feels oneself genuinely active and not the plaything of external influences, the slave to one's own passions. We avoid them, we avoid sensory delusions, error and immorality, when our behaviour is determined by our own appropriate ideas, by the rational part of our being. The superiority of thought reveals itself in the fact that it is thought which allows us to draw a distinction between action and passion. Only the wise man is free, because his action is autonomous. But what must he do when in doubt or hesitation? Without the guarantee of self-evidence, without being motivated by clear and distinct ideas, may he commit himself? Descartes recognised that "actions in life generally tolerate no delay, and it is an indisputable truth, that when we are unable to recognise the truest views, then we must follow the most probable" (*Discours de la Méthode*, Part III). This is also the reason why he establishes a clear distinction between theory and practice, between what is suitable in the search for truth and what is to be recommended in the actions of life. But is it not understandable that within this prospect the wise man will increasingly renounce action, the conditions of which seem to him uncertain, and prefer instead a contemplative and mystical way of life, for which he will, if necessary, prepare himself by ascetic and purificatory practices?

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The two philosophical trends which we have characterised schematically are as diametrically opposed in their theory of knowledge as in their theory of action.

For the classical tradition, true knowledge and free action consist in conformity with an order existing prior to all human action. Human interference is a cause of subjectivity, that is to say of error and immorality. The disagreements of men, their discrepancies throughout history, are equal proof of the imperfection of their knowledge and of their action. A preliminary ascetism is required to repel everything which will estrange them from the universally valid order. Everyone must free himself from his passions and prejudice, from everything that carries the stamp of his personality and environment. The best way of guiding our reason consists principally in a purificatory discipline, which leads one to attach oneself only to clear and distinct ideas; these are understood by means of evident intuitions which guarantee the truth of their object.

Knowledge develops from certainty to certainty, while it follows the good order which progresses from the simple to the complex. Only knowledge elaborated in such a way deserves the name of science. Infallible understanding will result from the proper use of reason — a faculty common to all normally gifted men, and complete in each of them.

A science founded upon rational intuitions would be a perfect image of the reality it describes. But, in order to justify this conception of truth as corresponding to reality, one must presuppose that the reality itself which is revealed to the intuition is composed of true propositions, independent of all human language. The true propositions of scientific language must reflect the very structure of the real in as transparent a way as possible, without ambiguity, and without confusion. The concepts employed must conform with the natural classification of the things.

Like the true proposition, the virtuous action conforms to an established order. The free action of the wise man will follow the objective rules of morality and natural law revealed to him by his reason. From St. Augustine to Leibniz, the Christian tradition of philosophy, including St. Thomas, Duns Scotus and Descartes, finds in God a guarantee to human thought. Since the divine intelligence knows in advance the solution of all problems, and makes these known to us by natural and supernatural enlightenment, Christian optimism gives confidence to the philosopher. The Christian rationalist knows that all problems have their solution within eternity, and to discover it he only needs to exercise skilfully his natural faculties. It is fully understandable why an atheistic rationalism cannot boast the assurance of a Descartes, and cannot advance the same intellectual imperialism. It must reduce the scope of its assertions to the single field to which exact science appears to provide a key, declaring the normative field, the rules of which should govern our action, as being beyond reason. Thought, capable of knowing the real, becomes incapable of justifying human behaviour rationally. One has to give up a science of ends, for these are alien to the real. Only a descriptive science can base itself on facts. That which ought to be not being the image of what is, there is no truth in the normative field. The latter eludes rational knowledge: the ideal of practical reason proves to be an illusion, if not a contradiction in terms.

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Whatever the difficulties of the classical tradition, and however insurmountable they may appear, it has least the advantage of having produced

coherent theories of truth, reason and freedom. As to the contemporary schools, for which neither truth nor freedom conform to a pre-established order, and which refuse to see in reason an immutable faculty, can these schools offer us satisfactory criteria for what is a valid thesis, a reasonable choice, a justifiable decision? This is, in effect, how questions relative to truth, freedom and reason must be formulated in a philosophy which asserts the superiority of action over thought. It is not enough to say that every assertion pre-supposes action; that we regard as true the theses on which we are prepared to base our behaviour; and that the success of the latter alone guarantees the appropriateness of our ideas: it is also required that we be able to point out the criteria of effective action and reasonable choice and to define the conditions of error and failure. All this demands a theory of practical reason, all the more indispensable because it must furnish at the same time rules of action and of thought. This theory moreover has to avoid the difficulties of classical philosophy.

I should like to discuss one or two suggestions which seem to me to provide a solution to the problem with which we are concerned.

In classical philosophy theories of knowledge have considered the individual as being alone in the presence of the universe. Whether the individual's thought develops as a result of his experience of the real, or whether reality is the reflection of rational thought, true thought appears to be that which corresponds to the real. But, in fact, interposed between the individual and the universe is his social environment with its traditions, language and technology. Before arriving at a personal outlook, each man has undergone a moral, political or religious education and has been initiated into one or other of the innumerable sciences and technologies of his age. *Ab initio* all knowledge is tradition, instruction and conformity. Not only language, common as well as technical, but also the rules and methods of verification and proof are elaborated by tradition and taught by initiation, prior to creative work. In addition to the rules and critical methods common to all exercise of thought, each discipline develops procedures and specific methods which enable it to discriminate between what is relevant and what is not in its proper sphere of research. In this way, each intellect is conditioned by its training and education. All ready-made knowledge, passed on from generation to generation, seems natural and conformable to the real; it raises scarcely any problems as long as rules and methods which have already been elaborated can be applied without difficulty to new situations. But if a difficulty does arise, if a problem presents itself where

all the known methods are unable to afford a solution, then a creative effort immediately becomes necessary. It becomes necessary to invent new procedures or to adapt old techniques, to formulate a new theory which will require the creation of a new terminology, to make the previous rules more flexible through modifying their fields of application. In the effort of adaptation to the new problems which result from circumstances or which the inventive intelligence of the scientist postulates, creative thought will modify or question the frame itself in accordance to which the situation had been first examined. Thus every new theory and every new practice, which tends to eliminate the imperfections of one or other elements of the cultural heritage, should demonstrate its superiority over that which it tends to replace, in a manner convincing to the minds trained in the discipline to which the new proposition belongs. This will rarely be a matter of formal demonstration, but an assessment of the advantages and inconveniences of the two theories or practices. In sciences and in technology the arguments will rely on such concepts as coherence, simplicity, clarity, fecundity, yield, utility; whereas in politics, morals or religious matters the following will be involved: freedom, justice, purity, faithfulness or sanctity. All these concepts are more or less indeterminate because the ways in which they are used are diverse. These ideas are defined and modified at the time they are used and it is this flexibility which prevents their formalisation. An argument which uses these concepts will appear the less immediately convincing, the more revolutionary the changes which it supports. One can more easily plead the cause of an innovation the less it disturbs our habits and the schemes of our thought. At their first onset revolutionary changes have rarely been accepted as part of the cultural heritage.

This process of evolution in our ideas and our techniques obliges us to reconsider, in terms alien to the classical tradition, the fundamental problems of knowledge and action, and most especially the concepts of truth and freedom elaborated in view of these problems.

Let us begin with the classical theory of truth as the correspondence of that which is said to be true with the object of the assertion. What does this correspondence mean when one believes that language is a human instrument, more or less adapted to the real and to the needs of communication with others? When we say that a proposition is true, do we not, at the same time, give an implicit judgment on the terms which are used? Let us imagine an account of an experiment by a sixteenth century alchemist in the language of his period; shall we say that his account is exact even if his terminology seems to us outmoded? Suppos-

ing that we could test the phenomena to which the account refers, will we not say that it is imprecise, mentions details which are superfluous and that it ignores important precisions, that it implies invalidated assertions and that we would have preferred an account in a less outmoded terminology. Shall we say that the present-day account would be truer than the old one? But if we use the word "true" in this sense, how can an intuition, by its self-evidence, guarantee for us the truth of a statement? If we set aside language, by which we describe this intuition, can we still speak of truth in such a situation? Is not truth relative to signs? And where there is neither sign nor meaning, can any other concern remain than a concern about data, which are neither true nor false?

It seems to me that all we can say, in this respect, is that to consider the truth of a proposition as indisputable amounts in any case to the presupposition that there is no need to discuss the terms of the language which furnished the means to express it; and very often, in fact, language is not at stake. But are we to conclude from this — whether we be realists or nominalists — that questions of language are alien to the definition of truth?

Personally I do not believe this. I believe that we are satisfied with the language employed, not because it is arbitrary, nor because it is the image of the structure of the real, but because it is convenient to us, at least till further orders: there is an implicit judgment of adherence to the language used and through it to the tradition by which it has been elaborated. When we wish to change the use of a term because it appears to us equivocal, or because it may encourage errors, or because it is based on a unsatisfactory classification, then we do not hesitate to challenge the propositions which until then had been considered true.

We detect the existence of a value judgment in terminological reform. Why then deny the existence of a value judgement in the use of tradition-accepted terminology? To renounce the exercise of a freedom amounts in a certain way to a use of this freedom.

In fact, freedom is not only adherence to a pre-ordained order. It is the choice of a line of conduct. We perceive this freedom when we depart from the automatic, from the routine, from the normal line of conduct. But in conforming are we not using freedom? The integrity of the judge who follows precedents is as much involved as that of the one who digresses. He has, nevertheless, an advantage over the latter in that he normally does not need to justify his behaviour. His decision, by virtue of the fact that it is traditional, does not need justification.

But the one who digresses from the norm seems to render himself more accountable, for he has to justify his initiative in order to make it appear rational. This justification must gain the support of those to whom it is addressed and whose habits of thought are being modified. Arguments will be used which will vary, depending on the frame in which the justification moves and which will furnish the criterion of their relevance. These arguments will be scientific, technical, legal, political or, philosophical, according to the frame into which they are inserted and the order which they seek to define or to modify. It is within this prospect that the idea of dialectical reason becomes understandable. Changes in the frame of reference result from the individual initiative of a creative mind, but for this initiative to be recognised, and for the resulting propositions to be absorbed into the frame which it modifies, reasons must be found which appear valid according to criteria that were recognised before.

To define truth and freedom only as conforming to an unchanging and perfect order means to support implicitly a theological vision which is that of classical metaphysics. But when the order is human and imperfect, to define truth and freedom as conforming to this order, does not mean to respect the absolute, but to respect tradition. To conform one's thought and action to the latter means to commit oneself, even if this commitment, owing to its conformism, appears to be normal and does not need justification. But because the order concerned is an order which men have created and perfected, and which remains always perfectible, the concepts of truth and freedom as conformity to an order reveal themselves as insufficient, and they must be completed by a concept of truth which allows us to conceive the superiority of one frame of reference over another and to understand why to abandon this latter frame and replace it by a new one constitutes a reasonable decision.

THE CRIMINAL AND THE SICK

by

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This paper deals with the everyday philosophies of ordinary men and women, as they are applied to certain recurrent situations, in this case, misfortunes. It does so from a sociological point of view, attempting to establish a few regularities, invariances, in social phenomena. We try to exploit the notion that just as philosophers are often trapped by their premises and their sense of logic, so are ordinary people caught when they construct models aiming at the understanding of other persons. If they say *a*, they often "have to" say *b*, or even *c*, on the basis of a sort of "everyday logic". Although this "logic" often is far from logical in any strict sense, we all have some familiarity with it. No attempt will be made to spell it out here in any detail. What we shall consider as being highly problematical is, rather, how people choose their basic premises when applying one or the other of a few ready-made models for the classification of other people.

It is our assumption that this choice is socially determined, in the sense that factors irrelevant to a scientific observer describing a person's actions play their part in the choice of everyday models for the perception of the same person. Furthermore, we propose that such a choice of model often implies a stand on traditional philosophical issues. We deal as sociologists with "man as a philosopher", thereby adding to the number of models of man which already range from "Zoon politicon", "economic man" etc. to "man as a personality", "man as an actor", "Homo Ludens".

* This paper developed while the authors were Fellows of the Center for Advanced Study in the Behavioral Sciences, Stanford, California. Sheldon Messinger's contribution is partly based on research carried out on the California prison system (The California Department of Corrections) with the aid of a Social Science Research Council pre-doctoral Fellowship.

"Man as a philosopher" is no entirely new subject matter to the social sciences. We can refer to the precedent set by the anthropologists who have cultivated the study of primitive theories of disease and other explanations of misfortune for a long time. They treat primitive man as someone who, in the face of tragedy, disaster and surprise, musters his intellectual resources and brings them to bear upon the situation in an attempt to re-establish order in the social universe, and to find grounds on which to act.¹ In a tentative and limited fashion we propose a similar sociological approach to modern man.

Crime and illness

One of the simplest sociological statements that can be made about the control measures invoked to deal with crime and illness is that they involve the exclusion of some sick persons and some criminals from the performance of their everyday social roles. When exclusion takes place we may view the control processes as having been initiated, and we may talk about entrance into the roles of the sick person and the criminal, since recognizable changes in rights and duties take place.

We are interested in exploring how a person gets into one of these roles rather than the other, and in the implications of the process of role-entrance for leaving the role. Although at first glance the way one enters the sick and criminal roles may seem too obvious to bear discussion, the patent fact is that in an increasing number of instances in modern society there is dispute over whether "criminality" or "illness" has led to a particular behavior sequence. This suggests the utility of holding as problematical the identification of a given deviant behavior as "crime" or "illness", and inquiring into the conditions of entering one role or the other in modern society.

Entering the sick and criminal roles

One manner of approaching the assumptions involved in identifying a performance as a crime is to examine the criminal law. From the sociological point of view, the criminal law is one among several regulators of values. The criminal law either orders priorities between values (ends) or between roads (means) to values. It gives the value of life to A priority over the value to B of getting rid of A, say by killing him. It gives the owner's road to his property priority over the thief's road to the same property. Although this is not unique to the criminal law,

any criminal law must necessarily give priority-norms about strivings related to values.

This inevitable characteristic of the criminal law bestows a necessary attribute upon any criminal. A criminal is someone who strives, or has been striving, towards a value. Whatever theories we may entertain about what he actually achieves, a necessary condition for his being a criminal is that he, in some fashion, was out to obtain a value. There are a number of ways to formulate this: that he was engaged in goal-directed behavior, that he made choices or decisions, or simply that he acted. The change that takes place in a person's situation when he becomes a criminal is due to a motivated act — a choice.

Medical science defines illnesses. It is no necessary condition of a definition of disease that it should be concerned with the inter-personal regulation of values and the ordering of priorities. The only necessary assumption about values in medical literature is that illness is negatively valued. Illness is against the individual's own good, at least from one point of view. One becomes sick by undergoing a change for the worse. Further, the presumption is that one becomes sick *in spite of* strivings for a value — attempts to stay healthy.²

Thus, to summarize and repeat: to become a criminal it is necessary to have appeared to be striving for a value. This is not necessary in order to become ill. In order to become ill it is necessary to undergo an apparent change for the worse.

Leaving the sick and criminal roles

We have held that the excluded criminal and the excluded sick are at a certain time in a fairly similar position, but that the necessary assumptions on which they enter their respective roles differ in at least one important respect: the entrance criteria are *explained* differently. Now we shall investigate some of the implications of these necessary, and necessarily different, explanations for the termination of exclusion:

One principle which will always in part determine exclusion of the criminal is the elementary protection of the values with which he was interfering. If he is excluded at all, some part of the exclusion will consist in separation from the values towards which he was striving. In other words, the defining characteristic of his crime is removed when the exclusion takes place. This separation of the individual from his defining situation does not, however, constitute recovery. Rather, there seems to be an inverse relationship between the distance from the defining value-

striving and the possibility of recovery. The relationship between the assumptions on which a criminal enters his role and the nature of his exclusion is such that his criminality cannot be falsified while he is in exclusion. One might say that in order to show any precise symptoms of recovery he must have recovered already.

There is no reason inherent in the qualifying characteristic of the sick person which makes it impossible for him to stay in proximity to the negative values defining his illness during the period of exclusion. On the contrary, it is necessary to remain in proximity to some of the negative values — fever, pain, etc. — as long as the exclusion is legitimate. When all signs of malfunctioning disappear, the sick person has recovered, and the role ceases to apply. The basis for falsifying the claims that the role criteria apply, or do not apply, is present throughout the period of exclusion.

Now, what we have been saying is not that it is more likely that a sick person will recover than a criminal. No general statement to this or to the contrary effect can be made on the basis of the above analysis. What we have implied, however, is that if the sick person recovers, the reality basis for consensus about the recovery is defined in such a way that it is relatively easy to obtain consensus. That is, role-entrance, recovery, and role-leaving are closely linked in the case of the sick. If the criminal recovers, on the other hand, there is no well-defined reality basis on which consensus can build.³ This may hamper “recovery” of the criminal, but it presumably may also speed up “recovery”. The essential point is that whatever criteria of recovery are applied to the criminal, they have no necessary relationship to the criteria of entrance into the role. Thus, it is not clear when — or if — the criminal role can be left once entered.

Recovery and the criminal law

Still, it may be asked, do not man-made laws furnish the predictability and consensus about the criminal that natural laws provide in the case of the sick? Is not the absence of accurate predictability of recovery on the basis of the nature of the crime simply an imperfection analogous to the lack of scientific knowledge within certain areas of medicine?

Our analysis presupposes that the difference is of a more fundamental nature. The crux of the matter seems to be that the function of man-made laws is, above all, even when they mete out punishments, to order value priorities. When predictability operates at all it seems to be of

the following nature: if A violates rule R, his crime is defined as being of a degree of badness B, which implies a time of exclusion T. As we know, this scheme of predictability has a limited application. It is limited to crimes handled by the courts, where court practices do not fluctuate very much, and only with regard to publicly administered exclusion. But the main point is that under any conditions the prediction seems unrelated to the problem of recovery, in the sense of disappearance of criminality.

The most convincing argument for the irrelevance of recovery predictions in the criminal law is that the law so readily substitutes fines for imprisonment, and that some modern systems of criminal law authorize imprisonment for periods far in excess of the life expectancy of any human being. In fines there is obviously no implication about the predicted time of recovery. Further, the idea of punishing people in terms of time-serving is a relatively new one; most systems of criminal law have done without sanctions meted out in time periods.⁴ The criminal law draws upon the future merely because under modern conditions time is one of the dimensions along which sanctions can be ordered. But no system of healing can work without time.

We shall claim that the relationship which the law establishes between a criminal act and a future time period is essentially of the same nature as the relationship established between a criminal act and a sum of money or a certain degree of physical pain, as in flogging. If this relationship is the same in all these cases, the criminal law cannot be concerned with predicting future events. We would hold, in fact, that the criminal law is basically not concerned with processes over time at all.

Crime, illness, and the situation of performance

We should like to present briefly what we take to be certain research implications of the above analysis. First, it follows from what we have said that where the entrance criteria of the sick and criminal roles are difficult or impossible to perceive, a deviant performance will be rendered ambiguous along the dimension of crime-illness. In other words, the understanding of a given deviant performance as crime or illness is linked to the situation in which the performance takes place, including the social characteristics of the performers and observers. This can easily be illustrated, although details of the process obviously await empirical investigation.

For example, in the case of the sick, or those claiming the right to

enter the sick role, our thesis suggests the following: any situation in which an individual stands to gain from withdrawal is such as to render suspect his claim to illness. Of course, this sort of proposition must be stated "other things being equal", and among those "other things" is prominently found the visibility of symptoms. However, within this proviso several situations can be immediately located, which suggest the truth of the hypothesis. The army stands as a situation, in general, in which the value of withdrawal, particularly for enlisted men, is so patent as to render claims to illness routinely suspect.⁵ Absences from schools, factories, etc., in short, any situation in which the observer ordinarily assumes that the person claiming illness may be motivated to withdraw, is such as to render claims to illness ambiguous. The reverse situation may be seen to obtain as well — that is, it is easier to be categorized as ill when the situation points to significant deprivation following validation of a claim to illness. For example, the child on his way to a party is suspect.

Our thesis also suggests that any situation in which ends of doubtful value to the performer of a deviant performance are achieved, is such as to render the criminal basis of the performance suspect. Petty theft by the wealthy is of this sort. Arson, except where insurance is involved, is another example of a situation in which the basis of "crime" seems as likely to be "illness" as "criminality". No doubt other instances could be cited. But what seems called for instead is the careful investigation of the nature of those situations in which involved persons have difficulty in deciding the nature of the deviant performance. One excellent source of information should be court records of cases in which a decision of "insanity" was reached (or was sought but not reached). Another source of information yet to be exploited should be legislative records of discussions leading to changes in the law providing that certain crimes, e.g. sex offenses, become an occasion for sending the performer to the mental hospital rather than to prison. On a still broader scale, we need insight into the social conditions which lead some people to regard prisons as properly mental hospitals.

All of these investigations will perforce deal with the typical actors and typical motives we use in structuring our everyday worlds. And such work has yet to begin in earnest.

The everyday models of crime and illness

From what has been said it would seem that the sick and the criminal are constructed as members of two rather different worlds, and do not

only represent variants of a common category of roles.⁶ The sick person is constructed as someone with characteristics along a dimension of time, while the criminal is constructed without any time perspective. The defining characteristics of the sick person are located in space, but the defining characteristics of the criminal, partly because of this discontinuity, have no location in space either. Is criminality, then, a mere chimera? No, but it is not a characteristic which is built upon the model of "physical reality", as are modern ideas of sickness. Physical reality is not, however, the only model according to which social objects can be constructed. They can also be constructed on the basis of our other basic source of experience, the "inner world".

The characteristics of this inner world are somewhat elusive, but a few of them stand out rather clearly. In our inner world the possibility of choice usually seems to be present. The relevant argumentation before the inner audience is in terms of good and bad, and not in terms of cause and effect until the means are to be chosen. The inner world is characterized, furthermore, by discontinuity in terms of time. The difference between the past, the present, and the future is not a question of degree, but a difference with qualitative implications. About the present and the future it is possible to make choices. About the past, as past, it is impossible. In contrast to this, basic schemes for conceptualizing physical reality apply to past, present, and future. Our inner world gives the present, the now, a unique significance, since it is the meeting point of a process over which we have no power of choice and one over which we have power of choice.⁷

From this it follows that the criminal is one among a very large class of social objects. The sick, on the contrary, seem to represent a much smaller, but probably expanding, class of social objects.

It seems commonly accepted that it is the "moral" model which gives to human beings their character of being social objects, or even of "being human". Only organisms to which actions, motives, rights and duties are attributed constitute parts of the social system. From this point of view the sick person is not initially a social object at all. We shall not put great emphasis upon what is the proper way to delimit the concept of "social objects". Whether the sick person is conceived as a non-social object (until he becomes a patient) or as a social object constructed in naturalistic terms does not greatly matter. We prefer the latter terminology.

The victim of a crime compared to the sick

A comparison of the victim of a crime and the sick, the victim of a disease, may add to the preceding analysis. There is in any society, it seems, a persistent need to "explain" misfortunes. Theories of sickness, both primitive and modern, belong in this category of explanations. For the reasons already given, it seems feasible in terms of our prevailing values to explain sickness in naturalistic terms, as a process which is just happening. We have also seen some reasons why the naturalistic model is inappropriate to the criminal and his behavior as a criminal. But how does the problem of explanation look from the point of view of his victim?

Since the crime, by definition, implies misfortune on the part of the victim, a change for the worse, there will be little spontaneous tendency to look for acts or choices made by the victim. Nevertheless, the criminal law, unlike modern medicine, does reckon with willful choices or provocations, on the part of the victims, as extenuating circumstances. One may put the difference between the sick and the victim this way: in medicine it makes no difference (at least until the advent of psychosomatic medicine) whether a disease is "deserved" or not. From the point of view of the criminal law, it does make a difference whether the victim "had it coming to him" or not. And the reason is that the criminal law deals with interaction situations and that values desired by both parties are relevant. Under such circumstances a symmetrical application of models is natural. If the criminal is viewed in normative terms, so is the victim. The victim must be innocent for the criminal to be guilty; or rather: the guilt of the victim reduces the guilt of the criminal. Comparisons of merit and guilt are relevant.

Granted that the normative (moral) model is applied to the victim, it fails to give "explanation" where the victim's innocence is clear. If he did not deserve his misfortune, why did it happen? Now the question "why" (but probably not its underlying psychological tension) changes meaning and becomes causal-genetic. The tendency will be to make the causal explanation very brief: I lost my money because a dishonest person wanted it and took it. Whereas the normative model "explained" by *comparing* guilts, this explanation *refers back* to a state of guilt in another person as a cause.⁸ We know there are other, longer causal chains by which the question "why" can be tentatively answered: thefts occur because there are people who, by deep-rooted social and

psychological factors, are lead to steal. This type of explanation has many merits, but it does not specifically explain why *I* became a victim. Criminological theories are theories of criminals and not theories of victims.⁹ It seems that about the only available explanation is that the criminal *chose* me as his victim. The explanation is certainly a shallow one, but it does carry the causal chain one step outside myself, which seems necessary if I do not want to accept blame for the misfortune.

There exists an additional "explanation", consisting in the assumption that the misfortune is due to *bad luck*, that it is an accident from the victim's point of view. And there are good reasons why this explanation would be more likely to prevail in the case of a victim than in the case of the sick. It has less to do with the positive merits of the "explanation" itself than with the alternative explanations which it pushes aside. For the victim to ascribe his misfortune to bad luck excludes the explanation that he deserved it. Furthermore, it excludes or modifies the explanation that the criminal chose *him* as a victim, which might also be disturbing. It seems that the sick person has less to gain by such an explanation and is so much the more open for the application of causal-genetic schemes, even though they may be very tenuous.

Interaction and exclusion in relation to the criminal and the sick

Known sickness seems virtually impossible without some exclusion. In almost all cases where a person is classified as sick, there will be certain relatively well-defined things he is not supposed to do or things he is permitted to abstain from. This is sufficient to justify the application of the role concept to his situation. With crime, the situation appears to be somewhat different. There are vast groups of crimes (or delinquencies) in both primitive and modern societies, for which the criminal law prescribes no exclusion and where no social ostracism may be present. In a large number of pre-literate and archaic societies the major sanction against crimes is the payment of damages.¹⁰ Although the nature of the pertinent actions makes it reasonable to classify them as crimes, they are not supposed to be followed by exclusion; but in the same societies some crimes lead to death, banishment or excluding mutilation. In our own society some crimes lead to exclusion, death, imprisonment, custody, etc. Others (the majority) lead only to fines;

and social ostracism is obliterated by the relative secrecy of both the offense and the penal sanction. In other words, criminality does not in itself automatically imply exclusion; this can also be expressed by saying that not all crimes are related to a criminal role.

Why is sickness universally related to exclusion, although in varying degrees, while crime may or may not lead to exclusion? We shall sketch one possible element (among several) in an explanation of this difference.

The sick person is unable to be healthy for the time being; however much he chooses to be well, he cannot. Failure to perform the roles he previously performed, or normally would have been able to perform, is not his responsibility. Viewed from any Alter's side, this means: if Alter does not exclude him to some extent in his interaction, by cutting down his expectations, the sick person will disappoint him. And the important point is that the normal everyday response to such disappointment is inappropriate because of the irresponsibility of the sick. A normative reaction to the disappointment has no balancing effect; it cannot correct the sick person's behavior; and, which is significant here, this failure has to be taken on as Alter's responsibility. He would thus easily get himself into guilt-provoking situations. In other words, protection of Alter's vital interaction interests demands that he cut down on his expectations in accordance with his understanding of the sickness. Such a reduction of expectations does, however, imply exclusion. By under-expecting, proper performance would be made socially difficult or impossible, irrespective of somatic ability. It is known that the proper balancing and timing of exclusion-levels is essential to the recovery process of the patient, showing that the implications of a naturalistic view of the sick and a normative view of him are mutually dependent.¹¹

The criminal is not perceived in terms of inability. On the contrary, he is usually perceived as having been able to act differently had he chosen to do so. This makes it easier in one respect to interact with a criminal without getting into an anomic state for which Alter has to take responsibility. Let us assume that someone who has committed, or is convicted of, an offense, is met in interaction without exclusion. Expectations are not reduced relative to what they would have been had no criminality occurred. Alter may be disappointed; although this is not likely to happen in the same way as with sickness, where stable expectations built up during a long time by close relatives, friends and associates may become unrealistic after the onset of sickness. A crime is a different kind of change, which need not affect the realism of long-

standing expectations on the parts of close Alters. Whether it does or does not depends more upon the nature of the expectations themselves. Let us, however, assume that the criminality in some sense renders normal expectations false, e.g. the expectations of law-abidingness and trustworthiness in the handling of agreements and contracts. Disappointment will ensue. But it is quite proper to react normatively to this disappointment, because no assumption of causal inability applies to the criminal. This also means that ignorance of the criminality does not vitally affect the appropriateness of response in the way ignorance of sickness may do. Whether the normative reaction to the criminal will correct his criminality is doubtful. But, unless causal-genetic considerations intrude, there will be no occasion for Alter to feel guilt about having misunderstood the situation or for having chosen an inappropriate mode of response. The everyday normative attitude does not seem to lead to anomie in these cases, as it would in relation to illness.

We can put it this way: to react properly to illness, one must either understand the sickness naturalistically or one must know normatively what the patient "can" and "cannot" do. To react properly to the criminal one does not have to know more than in any other interaction-situation. We may also say that Alter always has the burden of proof in interaction with a sick person. In interaction with the criminal, Alter has no such burden of proof. In the case of conflicting definitions of the situation the criminal has the obligation to prove that Alter acted on false assumptions. Alter is supposed to act as if he had no knowledge of criminality, if the crime has been dealt with by the proper authorities. The function of the lay (non-professional) environment is reversed when we go from sickness to crime. The environment is supposed to understand the sickness but forget the crime.

From what has been said it follows that the requirements of interaction necessarily demand some exclusion in the case of the sick but not necessarily in the case of the criminal. This lies in the naturalistic constitution of the sick and in the normative constitution of the criminal. Although the *necessary* characteristics of a criminal are compatible with no exclusion, many criminals have additional characteristics that for different reasons demand exclusion, and sometimes more vigorously than in the case of any sick person. For one thing protection of values may demand removal of the criminal from the opportunity to approach them.

The repercussions upon social status of illness and crime

We have so far dealt with the excluded state of the sick and of the criminal as if they were of a similar kind. They are similar in so far as both the role of the patient and the role of the convict or inmate are incompatible with other role performances. They differ, however, not only in entrance and termination criteria, but also in the *kind* of incompatibility implied.

When the criminal is excluded, especially when he is imprisoned, the criminal quality will tend to spread to the whole person in spite of the Courts' tendency to deal specifically with the criminal act.¹² Prisons, unlike fines, demand the exclusion of the total person, irrespective of the specificity or generality of the judgment authorizing the imprisonment. Since the criminal is held responsible for his act, he will also tend to be blamed for his total criminal status. In other words, he is perceived to have chosen a criminal role in society. And this general choice (which is an imputation, not necessarily a fact) is considered to be more or less incompatible with a number of legitimate roles which the criminal may previously have held; with being a "good family man", "a citizen", "a political leader", "holding a responsible job", "being in a respectable business", "being an honest worker". It may be assumed that the feeling of incompatibility will be the stronger, the more highly desirable incumbency of a previously held role is supposed to be. In other words, to be a criminal is considered least compatible with the high status and high power positions in society.¹³ The perceived incompatibility, if sufficiently strong, may result in one or both of two consequences: a) the individual criminal loses his status because of his incumbency of a criminal role; and b) the criminal throws doubt upon the moral quality on the basis of which the position of his status peers is founded.

Both of these consequences, but especially the latter, may disturb the system of stratification in a society; and remedial institutions are likely to arise. A simple solution is for laws or customs explicitly to define crimes and sanctions differentially for offenders of high and low status. Primitive customary law sometimes implies that what is forbidden for ordinary people may be permissible for the chiefs. Or it may state that what is proper sanction for an upper class offender, differs from the proper sanction against a lower-class offender. The criminal laws of Europe during the late medieval ages stipulated different sanctions for high and low status offenders. Many codifications made it possible for

the offender to negotiate about payment of damages instead of being punished. Only the upper classes were able to pay such damages. The consequence was that there existed one criminal law for the rich, where the fine was the major sanction, and another criminal law for the poor based upon capital and corporal punishment.¹⁴ Schwarzenberg's *Peinliche Halsgerichtsordnung* from the 16th century states that criminals who were "ehrbare" (honorable) were to be fined, while those who were not, should be sanctioned with corporal punishment. The distinction was, as practised, in terms of social status. English criminal law from the 16th and 17th centuries stipulated more severe punishment for those who could not read than for those who could.¹⁵ As late as in the 19th century German criminal law instituted a privileged kind of imprisonment, "Festungshaft", for offenders belonging to the upper classes.

With the growth and spread of democratic, egalitarian ideologies and of achievement as the predominant basis for status, explicit discrimination in the criminal law became untenable. One of the achievements of the great criminal law reformers of the period of enlightenment, Beccaria, Feuerbach and Bentham, was to give everybody an equal opportunity, in principle, to become a criminal. "The criminal" became an open role, depending upon negative achievement. They left it to the agents of enforcement to cope with the problem suggested above. The problem has in part been taken care of by the increasing use of fines, which are usually meted out secretly (without public trial) and which tend to be viewed in specific terms, related to the act more than to the person.

We shall advance the hypothesis that another way to cope with possible conflicts between criminality and status is to institute individualizing treatment of serious offenders, culminating in the idea that criminals are often sick people, and should be dealt with accordingly. The development of this idea has many roots; and our hypothesis claims no more than that the need to cope with incompatibilities between criminal roles and certain status considerations has been a supporting factor in this development.

The sick criminal is not considered responsible for his crime, and probably even less for having chosen to become "a criminal". Although it has become apparent that he is incapable now of performing other roles, especially the high status roles, he is not morally blamed for the incompatibilities in his situation. And the idea that he was "exploiting" his high status when committing the crime, an idea which easily may reflect upon his status peers, is not likely to arise. The secrecy which follows when mental sickness is indicated, further tends to protect the

criminal and his environment from observations that might lead to status-challenging moral judgements. The assumption about sickness, that it is temporary and provides for a recovery process, means that for the sick criminal there is a definitive way back to his lost status, although he may not be able to fulfil this expectation of recovery.

If the historical hypothesis outlined above is correct, we should assume that in current criminal trials there is a tendency to label upper-class defendants as sick more frequently than lower-class defendants. Such a discrimination would also follow from the assumption that criminals are out to obtain values. Since the high status offender is characterized by the higher general accessibility of values to him, it seems less natural to perceive him as somebody striving for a value through a crime. In addition to this, he appears to have more to lose by being caught, which renders his action even more irrational in terms of striving towards values.

If we think in terms of empirical comparisons of upper-class and lower-class offenders, however, we must take note of a factor which contributes to a discrimination in the opposite direction. In most modern societies there exists a group of offenders who outwardly are less characterized by specific illegal acts than by a general sordid state of existence, the hard core of which are the alcoholic vagrants. In terms of the criminal law they strive towards values by illegal means and interfere with the values of others. But they do so in a blurred way. What is highly visible about them is that they are generally unsuccessful in obtaining the values desired by most people, a home, decent clothes, regular meals, a good income, etc. They give the impression of being down and out, often showing signs of somatic malfunctioning due to drink and an unhealthy way of life. Small wonder, then, that this has been one of the first groups of offenders — in Norwegian law — to be defined as sick. Closer analysis has shown, however, that the legal authorities have failed to take full cognizance of the everyday logic of the models they deal with.¹⁶ Alcoholic vagrants have been defined as sick in the sense of being in need of extensive, long-term "treatment". Since this treatment is in their own interest, certain guarantees of due process have been waived and so are ordinary considerations of proportionality between offence and sanction. But the vagrants are not treated as sick in the sense that they may choose whether they want to be treated or not, nor in the sense that they are submitted to medical intervention with a clear-cut therapeutic rationale. As a matter of fact, they are put away for long periods of time in old-fashioned penal institutions.

The transformation of inmates to patients

Another hypothesis about the evolution of the conception that criminals may be sick should be mentioned here. But let us again give a reminder that we are only dealing with partial, or supportive, explanations. Above all, we do not want to disparage the notion that the development of medicine, psychiatry and psychology in itself furnishes an important explanation of the idea that criminals may be sick. With this in mind, however, we shall exploit the notion that application of a time perspective to the criminal is a relatively recent development. It had its origin in conditions unrelated to the evaluation of crimes as such.¹⁷ Once it becomes usual, however, to sentence criminals to serve time periods in prison, it also becomes "natural" to deal with criminality as if it were a state capable of manipulation or modification over time. The criminal can be reformed. To be sure, the idea of reform was in the beginning conceived of as a moral change, a conversion or a gradual moral enlightenment. Solitary confinement was introduced with the rationale that it would give the criminal a chance to repent and think over his sins and make new and better plans for his future life, or prepare for the hereafter. It was inevitable, however, with increasing secularization and rationality that the process of reform should be viewed as a question of improvement or recovery in more ethically neutral terms. The analogy with the sick was near at hand, and probably the only one available.

Our hypothesis is, then, that the mere fact of criminals being held in confinement for specified periods of time, within rational systems of thought almost inevitably implies — in terms of everyday logic — a question of the functioning of time in prison as a process of recovery. Once this step is taken, it is inevitable that attempts should be made to apply the role of the patient to the inmate. The obvious suffering of the convict and his helplessness when confined make this step seem "rational" and "natural". That there are formidable obstacles to this development, however, has already been argued above.

It is hard to see how a hypothesis like the one cited above could be tested directly. Indirect evidence is found in the fact that no one seems to have proposed to turn those offenders whose behavior is customarily sanctioned by fines into patients.

Underlying these various reasons for defining crimes as symptoms of disease, we may discern a general need for predictability.¹⁸ The

criminal law tells us nothing about what we can reasonably expect of a criminal in the future, unless it stipulates a death penalty or long-term imprisonment. Such sanctions are becoming increasingly unpopular, leaving us with more uncertainty concerning the future behavior of those who have once been convicted of a crime. In this situation we find the development of a whole new science, criminology, which attempts to link the individual criminal act to processes that have a time dimension, and which furnish measurable stages allowing predictions to be made. In terms of scientific premises and procedure, criminology is puzzling; but the puzzle is diminished if one assumes a social need for predictability in cases where the group has felt threatened or bewildered by a visibly deviant act.

The simplest way to link an offence to a predictable process is to connect it with relatively stable, although not necessarily unchangeable traits in the offender. The development in these schemes of interpretation has been from inborn organic traits, Lombroso's deformed and born criminal, towards personality defects or defects in enduring social stimulations. Since these theories get their support from a social need, they have blindly assumed the dictate of everyday logic that like causes like. A negative consequence (the offence) must have a negative cause, a defect in the offender. In other words: the offender must be sick or he must be in a state much like a disease. Such a definition brings his disturbing display of deviance in touch with elaborate schemes of prediction based upon naturalistic models of persons. In principle, his behavior is predictable. To what extent this "predictability in principle" will be achieved in practice by successful treatment and other precautionary devices, remains to be seen.

Implications for modes of decision-making

We have discussed some differences between two classes of social objects as they are perceived by others, and we have indicated some consequences of these for the interaction between the deviant and his social environment. The decision concerning the entrance, termination and type of exclusion and treatment is, however, often made by professionals on behalf of society. The doctor and the lawyer, especially the judge, are the roles to which is delegated the authority to make these decisions. Decision-making within medicine and law show some striking differences, which are related to the differences between the models around which the deviant roles of the sick and of the criminal are constructed. We

shall briefly sketch some of these differences between medical and legal decisions. Such a juxtapositioning may throw some light upon more general problems of decision-making and upon the sociological conditions for the application of science, since medical decisions are derived from scientific, utilitarian thinking, while legal decisions are not.¹⁹

Medical decisions

Most decisions of the scientific-utilitarian type concern interference with nature, the relationship of man to his non-human habitat. Decisions of this kind have indirectly had the broadest possible consequences for inter-personal relationships and for the whole structure of society. But our concern is that area of science which only deals with nature in the sense that within it man and human inter-relationships are understood to be a part of nature.

The decision to be made by a doctor is, as a rule, how the patient is to be treated. In this decision we can distinguish three elements: the diagnosis, prognosis and choice of therapy. Let us consider the decision-maker's relationships of responsibility, to whom and for what he is responsible.

The doctor is primarily responsible to the patient. The patient normally comes voluntarily to the doctor and asks him to make a decision which it is in his own, the patient's, interest to obtain. The doctor is also responsible in other directions for the way in which he makes decisions. He is responsible to the medical association, and may incur its disciplinary measures. He may also be made responsible to a court of law in the case of gross neglect of duty, and may be punished. But the latter responsibility is almost entirely derived from his responsibility to one particular patient or to a general group of potential patients.

For what the doctor is responsible is not so easy to define. If we take responsibility in its widest possible sense, so that it includes everything that influences the doctor's reputation, salary, career, prestige etc., one must conclude that the doctor is primarily responsible for the future effects of his decision. A cure will be a central indication for both doctors and patients that the doctor satisfied the expectations one had of him, although it is not the only criterion. In spite of exceptions and modifications, the doctor's responsibility is to a great extent linked with the results, the effects of his decision. This is shown above all in the fact that the doctor is considered in duty bound to check the effects by following the process of cure, and also possible negative developments.

It is characteristic of medical decisions of some importance that only those with a certain professional training have the right to make them. The law of quackery forbids others to make such decisions, although it is not rigorously enforced when lay healers deal competently with minor ailments. As soon as more important decisions in serious cases are taken by people without medical training, however, a hue and cry is raised which shows clearly that central norms for methods of decision-making have been ignored.

Medical decisions are secret, and this is laid down by law in the rule concerning a doctor's pledge of secrecy. This applies to the publicizing of diagnosis and therapy in concrete cases, but it is also up to the doctor's judgment as to how much he will tell the patient about the basis for his decision.

Medical decisions are limited to problems of health. The doctor cannot include in the appraisal of his decisions any other effects they may have. Neither is he responsible for any effects beyond the state of the patient's health. This is clearly brought out in the doctor's duty to help enemy soldiers in time of war. It also holds good in cases when it is necessary to give medical help to condemned criminals or dying persons who have little to look forward to. Nor can it be considered his right or duty in such cases to assess the patient's total sum of happiness. His decisions concern the patient's health; and it is the effect on his health for which the doctor is responsible. Problems may arise here if the doctor, in his capacity as medical officer of an industrial plant or an army camp comes up against conflicting pressure from considerations of health and of the organization's efficiency. There seems to be a strong tendency amongst doctors to insist on limiting their responsibility to matters strictly concerning health.

A doctor's decisions in a particular case of illness do not need to be similar to decisions taken in other cases. Each individual case must be judged according to its own peculiarities. A doctor may know that a number of cases presenting particular symptoms, stomach pains, constipation, etc. have been diagnosed as catarrh and been given treatment accordingly with unfavourable results; but this is no reason for not diagnosing cancer and treating it as such. Or to put it more precisely, other similar cases are only to be treated as material for experience, data for generalization, but they do not have the character of precedent. The doctor must do his best in each individual case for each individual patient without taking into account whether other patients, attending other doctors, or in other circumstances, are not being given similarly

thorough diagnoses and equally good treatment. When the doctor is dealing with a patient, justice is not a particularly relevant consideration. Equality before medicine is not an ideal of the same kind as equality before the law. It is the duty of the health authorities, not of medical science nor of the doctor, to think of justice. From the point of view of the doctor every patient has, in principle, the right to unlimited good service, irrespective of whether other patients can obtain the same.

Legal decisions

"Juridical man", man as the organs of the law regard the parties in a case, is not an organism which goes through processes of change over a period of time. His criminality is not something that can be established by investigating him at the moment of the decision. The criminal's qualification for being a criminal depends entirely on an historic fact, his guilt.

This brings us to the question of the responsibility of the legal decision-makers. Towards *whom* are they responsible and for *what*? The judge is not only responsible towards his immediate "clients". That is quite obvious. His responsibility towards them is almost entirely derived from other relationships of responsibility. He has no particular responsibility for their welfare if its furtherance does not coincide with the judge's duties towards other instances. The judge's relationships of responsibility are somewhat abstract, and are linked with the law more than with the persons who make laws.

There is naturally a connection between this relationship and the fact that people may become the objects of legal decisions without having asked for them, indeed, if necessary against the strongest opposition. This is the situation, with few exceptions, in criminal cases; and in civil disputes also it is fairly usual for one of the parties not to desire any legal decision. But in the latter case the situation is nevertheless such that at any rate one of the parties, the plaintiff, asks for a decision in the same manner as a sick person. This does not, however, create any particularistic responsibility for the judge. The plaintiff does not become the client of the judge in the same way as the sick person becomes the patient of the doctor. And while the doctor receives a fee from his patient, similar fees from the plaintiff are called bribes, and represent a punishable relationship.

For *what* is the judge responsible? What features of the decision influence the judge's legal and social status? The judge is responsible

to a much lesser degree than in medical decisions for the decision's future consequences. The judge's responsibility is primarily retroactive, linked with factual conditions in the past. He has the responsibility of describing these in a true fashion; and he has the responsibility of forming a reasonable estimate of the degree of probability of the presence of guilt or of other legally relevant characteristics of past actions. If this probability is not sufficiently strong, he must avoid building on his interpretation of the facts, and decide the case instead on the basis of rules concerning the burden of proof, rules concerning which of the parties is to bear the risk if a fact remains insufficiently clarified.

Along with the responsibility for describing correctly certain facts which belong to the past, the judge is responsible for the correct choice of rules under which the facts are to be subsumed. There is here more certainty and less room for individual judgment than in the doctor's choice of medical text under which he can subsume a case. If the judge gives a correct description of certain facts in the past and applies the right rules to those facts, he is safeguarded quite independently of the factual consequences of his decision.

That the judge has little responsibility for the effects of a legal decision is based on the philosophic principles on which our legal thinking builds. While the action of the doctor is often assumed to create health and to be the cause of the patient's cure, it is not usual to assume that the judge creates law, rights and duties. The parties in a legal case have themselves already created their rights and duties by actions in the past. The judge only *establishes* the content of these rights and duties which are assumed to have existed before the judge was called upon. We might also say that responsibility for the real consequences of his decision, if it is formally correct and retroactively plausible, is assumed to lie on the parties to the case. The judge merely establishes what they have done, and how this is judged by abstract rules of law. The real consequences are implicit, from the legal point of view, in the parties' own actions. We might perhaps say that medical ethics takes the responsibility for the future from the patient, the sick person, and places a considerable amount of it on the doctor. But legal ethics places the responsibility for the future on the law-breaker or on the litigating parties, and exempts the judge from it to a very great degree.

Contrary to cases of medical decision, laymen play a fairly large part in legal decisions. There are even countries where permanent judges

are appointed by election, and these can be persons who do not have a legal training. To the same extent as quackery is condemned in medicine, so the institution of the lay judge is honoured in the administration of justice. It doubtless has something to do with the element of force in law. In medicine "the people" sanction decisions directly, in that the patient agrees to be treated. Since this direct agreement is lacking in many legal situations, another, indirect, form of control by "the people" is necessary. Because the law is an aspect of the state, the judicial process has come to be influenced by general political developments, where representation and democracy are ideologically basic demands.

The principle of the lay judge naturally has a number of consequences for legal decisions. As long as the lay judges are included the courts cannot draw much on specialized knowledge. This also means that legal thinking and terminology have to maintain the possibility of communication with laymen. It may also have the effect, which is its expressed purpose, of preventing judges' decisions from being too strongly influenced by the special attitudes and ways of thought which obtain among jurists. Traditionally, jurists have been a somewhat unpopular professional group. The profession cannot under any circumstances measure up to "the men in white" who have been strongly romanticized.

The element of the layman in law is not only apparent in the institution of the lay judge. The professional judges are also more or less lay persons in a scientific or technical sense as regards most of the questions put before them. Few judges in Norway are specialized as to factual area of competence. One and the same judge will usually function in cases which stem from a number of highly varied walks of life, and which are perhaps linked with several different sciences.²⁰

The demand that the law shall be *one*, the same for all, becomes more difficult to meet, the more specialized the courts are. One could perhaps go a step further, beyond the administration of justice itself, and maintain that, because they are non-specialized, the courts provide a point of contact for problems arising from areas of dissimilar specialists. In this way the lawyers become a link between the many professional "subcultures" which are to be found in a modern industrial society. The lawyers, and in the last resort the courts, help to prevent these special cultures from becoming too greatly isolated from each other, an eventuality which would cause the social structure to creak at the joints.

Oddly enough it is perhaps also true that professionals are not always most suited to making certain decisions even in their own field of specialization. Here we are thinking of those decisions in which there

is a strong element of doubt, and legal decisions often belong to this category. This doubt would often make it impossible for a professional to reach a decision at all. His special insight and professional interest might make it psychologically exceedingly difficult to maintain that a particular decision, which in addition has to be reached quickly, is *the right one*. Lawyers are a professional group trained in making decisions, regardless of whether their scientific foundation is well worked out or whether they find themselves in an area where science is silent. Lawyers answer the need of society for a group of persons who can, and who are willing to, make — and justify — decisions in cases where the possibility of reaching a “scientifically correct” result is very small.

Medical decisions are on the whole secret, legal decisions are in principle public. This is to a great extent a consequence of the same considerations which support the institution of the lay judge. Since the individual can be forcibly exposed to legal decisions, there is little control in the fact that the accused himself knows the basis for the decision. Others must know it and make up their minds. In the same way as with the principle of the lay judge, the publicity surrounding legal cases also prevents too specialized and scientific a method of decision-making. There are three characteristics of scientific methods of approach which do not harmonize with complete publicity concerning decisions: the technical language of science, which is difficult to understand; its lack of engagement in important values at certain stages of the investigations, and finally its probabilistic character which openly warns against certainty and faith. As long as publicity is regarded as an important value in connection with legal decisions, it will be difficult for these decisions to take on these characteristics. And this means that it is difficult for them to take on a scientific character.

Legal decisions are not factually limited in the same way as medical decisions. But they too have a limitation, often a very narrow limitation. The judge can ignore everything which is “legally irrelevant”. He does not have the duty of stressing all those factors which he believes to be important for the consequences of his decision, but only those which the law—legislation, custom, and precedent — have indicated as relevant. These factors may belong to the most varied professional areas, but, in that their limitation simplifies reality and makes it possible to act, they have something in common with an empirical theory.

In legal decisions precedent is of great importance, and comparisons are relevant to the highest degree. Equality before the law is a basic legal value in the sense that similar cases are to be treated similarly,

even though several investigations have demonstrated the difficulties of realizing this ideal in legal practice. That the judicial system is built up as *one* pyramid, with *one* highest legislative instance and *one* supreme court, serves the purpose of guaranteeing a certain uniformity in legal practice. Nothing completely similar exists, and especially not in principle, with regard to medical or other scientific decisions. Scientific ethics rejects the idea of *one* supreme scientific authority. The centuries-long fight against the authority of Aristotle has created an exceedingly strong conviction on this point.

NOTES

- ¹ A social-psychological account of related problems in modern society is given in: Army Medical Research and Development Board, Office of the Surgeon General, Washington (Research Staff: Dembo, T., Ladien, G., Wright, B. A.) *Adjustment to Misfortune*, Final Report, April 1, 1948.
- ² For more detailed statements on the sociological concept of illness, cf. Talcott Parsons, *The Social System*. Glencoe, Ill. 1951; Aubrey Lewis, "Health as a Social Concept". *The British Journal of Sociology*. Vol. 4, 1953. pp. 109-24; Lester S. King, "What is Disease?", *Philosophy of Science*, Vol. 21. no. 3, 1954. pp. 193-203.
- ³ The problem is related to Festinger's distinction between "physical reality" and "social reality". Leon Festinger et. al., *Theory and Experiment in Social Communication*. Ann Arbor. 1950. Cf. also Leon Festinger, "A Theory of Social Comparison Processes". *Human Relations*. 1954, 7, pp. 117-40.
- ⁴ Georg Rusche and Otto Kirchheimer, *Punishment and Social Structure*. N.Y. 1939.
- ⁵ Roger W. Little, "The 'sick soldier' and the Medical Ward Officer". *Human Organization*, Vol. 15, no. 1, 1956, pp. 22-24.
- ⁶ For a somewhat different approach to the dichotomy, cf. Antony G. N. Flew, 'Crime or Disease'. *The British Journal of Sociology*, Vol. 5, 1954, pp. 49-62.
- ⁷ Cf. Alfred Schuetz "On Multiple Realities". *Philosophy and Phenomenological Research*. Vol. 5, 1944-45, pp. 533-76; Hubert Griggs Alexander, *Time as Dimension and History*. The University of New Mexico Press, Albuquerque, 1945.
- ⁸ Our theories of victims are closely parallel to a major type of theories of disease in primitive societies. Cf. Henry E. Sigerist, *A History of Medicine*. Vol. 1: *Primitive and Archaic Medicine*. New York 1951.
- ⁹ Cf. however Hans v. Hentig. *The Criminal and his Victim*. Yale University Press, New Haven, 1948.
- ¹⁰ E. Adamson-Hoebel, *The Law of Primitive Man*. Harvard University Press, Cambridge, Mass. 1954.
- ¹¹ Cf. Fred Davis, "Definitions of Time and Recovery in Paralytic Polio Convalescence". *The American Journal of Sociology*. Vol. 61, 1956, pp. 582-7.
- ¹² Cf. Vilhelm Aubert, "Legal Justice and Mental Health". *Psychiatry*, May 1958, pp. 101-13.
- ¹³ This problem is dealt with more extensively in Edwin H. Sutherland, *White Collar Crime*. New York, 1949; Vilhelm Aubert, "White Collar Crime and Social Structure". *The American Journal of Sociology*. Vol. 58, 1952, pp. 263-71.
- ¹⁴ Rusche and Kirchheimer, *Op. cit.*
- ¹⁵ Leon Radzinowicz, *A History of English Criminal Law and its Administration from 1750*. London 1948, p. 140.
- ¹⁶ We build here upon unpublished work by the Norwegian sociologist Nils Christie.
- ¹⁷ Rusche and Kirchheimer, *Op. cit.*
- ¹⁸ This need for predictability in relation to mental illness has been discussed in Elaine and John Cumming, *Closed Ranks: an Experiment in Mental Health Education*. Harvard University Press. Cambridge, Mass. 1957.
- ¹⁹ For a more extensive treatment, see Aubert, "Legal Justice and Mental Health".
- ²⁰ When the Chief Justice of the Norwegian Supreme Court retired this spring, he mentioned the manifold of diverse cases, ranging over the total span of human existence, as one of the greatest attractions of the Bench.

ON THE VERIFICATION OF STATEMENTS ABOUT ORDINARY LANGUAGE¹

by

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In this paper I shall discuss certain difficulties which seem to me to stand in the way of understanding or properly appreciating the work of the so-called "ordinary language" philosophers. These difficulties concern the interpretation of the various seemingly factual statements which such philosophers make about language. I am mainly interested in the question of how one would go about verifying these statements; insofar as meaning is bound up with verification, this is also a question of their meaning. Of course, it is possible to pretend that no clarification is required at all, that the sense of assertions about the ordinary use of language is perfectly obvious, or at least sufficiently so for the purposes at hand. But I do not think that such optimism is justified. Even among those who can claim to be "in the know" or to "get the point" there are wide disagreements both as to the truth and as to the meaning of given assertions of the sort under consideration, and these disagreements are by themselves a basis for scepticism. When in addition it is seen that such assertions play a crucial role in the discussions which are supposed to answer, dissolve, or somehow get rid of the traditional problems of philosophy, a philosopher may perhaps be excused for looking at the matter a little more closely.

First, a parenthetical remark about terminology. As is always the case when one attempts to formulate objections to a philosophical position of scope and subtlety, there is here the problem of finding a convenient vocabulary which does not beg too many questions. I shall make use of such terms as "analytic", "synthetic", "meaning", "sense", "denotation", in the hope that I am not thereby drawing a number of red herring across the trail, and in the belief that if necessary I could reformulate my points without the use of these terms, though with a considerable loss

of compactness. For instance, when I ask whether a given statement about ordinary language is normative or descriptive, and whether it is analytic or synthetic, I am not asking questions to which the alleged difficulties with the fringes of these terms are decisively relevant. Instead, I merely wish to know with what attitude the given statement is to be confronted; for instance, is it to be regarded as a recommendation, so that an appropriate query is "What will be gained by following this advice?", or is it to be regarded as true or false? And if it is to be taken as true or false, does the author intend it as true by virtue of the meanings of the terms involved, or does he consider himself to have made a statement the truth-value of which depends upon matters of fact? The answers to such questions will fundamentally affect our understanding of the given statement, and they do not, so far as I can see, need to involve us in any of the philosophical tangles which are supposed to be connected with the terms "normative" and "analytic". I believe that somewhat analogous comments apply to the other cases in which I make use of notorious philosophical terminology.

It will be useful to have before us some examples of the type of statement under consideration. Accordingly I quote a passage from Prof. Gilbert Ryle's treatment of the Freedom of the Will:

"It should be noticed that while ordinary folk, magistrates, parents and teachers, generally apply the words 'voluntary' and 'involuntary' to actions in one way, philosophers often apply them in quite another way.

"In their most ordinary employment 'voluntary' and 'involuntary' are used, with a few minor elasticities, as adjectives applying to actions which ought not to be done. We discuss whether someone's action was voluntary or not only when the action seems to have been his fault . . . In the same way in ordinary life we raise questions of responsibility only when someone is charged, justly or unjustly, with an offence. It makes sense, in this use, to ask whether a boy was responsible for breaking a window, but not whether he was responsible for finishing his homework in good time . . .

"In this ordinary use, then, it is absurd to discuss whether satisfactory, correct or admirable performances are voluntary or involuntary . . .

"But philosophers, in discussing what constitutes acts voluntary or involuntary, tend to describe as voluntary not only reprehensible but also meritorious actions, not only things that are someone's fault but also things that are to his credit. The motives underlying their unwitting extension of the ordinary sense of 'voluntary' . . . will be considered later . . .

"The tangle of largely spurious problems, known as the problem of the Freedom of the Will, partly derives from this unconsciously stretched use of 'voluntary' . . ."²

These, then, are examples of the sort of remark I wish to consider. They may not be the best examples that could have been chosen, but they do have the not inconsiderable merit of being relatively lucid. Prof. Ryle seems here to assert that the ordinary use of the word "voluntary" is quite different from the philosophic use. He does not mean merely that the ordinary man seldom talk philosophy. The difference between the ordinary use and the philosophic use hinges rather upon the alleged that the ordinary man seldom talks philosophy. The difference between sively to actions which ought not to be done, while the philosopher stretches it to cover meritorious actions as well. Now although the question whether Prof. Ryle is right or wrong in this particular case is not essential to the point of the present paper, I will say at once (1) that I very much doubt whether as a matter of fact the ordinary man does apply the term "voluntary" only to actions which (he thinks) ought not to be done, and (2) that even if this were shown to be the case, it would not have decisive relevance to a determination of the ordinary use of the word "voluntary". Some other factor, such as perhaps a disposition on the part of the ordinary man to talk more about things of which he disapproves than about things of which he approves, could by itself account for a relatively frequent application of the word "voluntary" to disapproved actions, even if the word were simply being used in one of its dictionary senses, e.g., "proceeding from the will or from one's own choice or full consent". In short, at first glance it seems that Prof. Ryle's assertion about the ordinary use of the word "voluntary" is false, and, moreover, that it is based upon doubtful evidence which would be entirely insufficient even if it were sound.

When one comes to so drastic a conclusion about an assertion seriously made, it is time to reconsider the possibility that one has not understood what was asserted. For further light we may consult an article entitled "Ordinary Language",³ in which Prof. Ryle attempts by means of a distinction between use and usage to indicate why empirical studies like those of the lexicographers or philologists are irrelevant to the truth of statements about the ordinary use of language. Such studies have a place in determining the *usage* of a word, he says, but not in determining the *use*. Uses are ways or techniques of doing the thing, the more or less widely prevailing practice of doing which constitutes the usage. He explains further that "ordinary use" is to be contrasted with

“non-stock use”, and that in general “use” contrasts in these contexts with “misuse”. Now it seems to me that Prof. Ryle fails to make good the alleged distinction between use and usage, and still less the point that through confusion in this matter philosophers have misunderstood the character of claims that the ordinary use of a word is this or that. Nevertheless, his discussion is not without significance for the matters we are considering: it appears to indicate that for him there is some sort of normative element in assertions about ordinary use. If the opposite of use is misuse, then use must be somehow right, proper, or correct. It is not easy to see how this can be applied to statements like those quoted about the word “voluntary”, but perhaps Prof. Ryle’s discussion in that passage comes down to an assertion that to predicate the word “voluntary” of praiseworthy actions is to misuse the word, presumably in some sense of “misuse” which cannot be defined statistically. But if this is the way the wind blows, it would be instructive to have an indication of the standards or goals with reference to which the term “misuse” is applicable. What authority deems it wrong to use the word “voluntary” as the philosopher does? What unwelcome consequences would attend the proscribed use? Surely the point is not merely that if you use the word “voluntary” just as the philosopher does, you may find yourself entangled in the philosophic problem of the Freedom of the Will.

Despite Prof. Ryle’s own explanations, I am reluctant to believe that the expression “ordinary use” is really a normative term for the ordinary language philosophers. The way in which they use it seems better explained on the hypothesis that it is a rough descriptive term, employed with little definiteness of intention, and that there is in addition a *belief*, not part of the meaning of “ordinary use”, to the effect that it is somehow wrong or inadvisable, or at least dangerous, to use ordinary words in ways different from those in which the ordinary man uses them. It is further supposed, and often expressly asserted, that in daily life words function well enough and lead to no great problems. In any case, although all forms of “nose-counting” are deprecated in these quarters, it often happens that when support is offered for an assertion that the ordinary use of a given word is thus and so, this support takes the form of an attempt to remind or convince us that the use in question is indeed quite frequent among “ordinary folk, magistrates, parents and teachers”. In other words, the statement is taken as having a factual basis and presumably as refutable by observation of the ordinary folk, magistrates, parents and teachers.

This brings me to the question of how one might verify a statement about the ordinary use of a word or phrase, when the statement is interpreted neither as a piece of advice nor as a claim that the use is sanctioned by authority. It is necessary here to make passing mention of a point of view, or rather a dodge, which cannot be taken very seriously. This is the comfortable suggestion that the average adult has already amassed such a tremendous amount of empirical information about the use of his native language, that he can depend upon his own intuition or memory and need not undertake a laborious questioning of other people, even when he is dealing with the tricky terms which are central in philosophical problems. Such a assertion is itself an empirical hypothesis, of a sort which used to be invoked in favor of armchair psychology, and it is not born out by the facts.⁴ It has been found that even relatively careful authors are often not reliable reporters of their *own* linguistic behavior, let alone that of others. (Indeed, this is hardly surprising in view of the fact that most of the time we use language in a more or less automatic manner). The weakness of the hypothesis is further revealed by the fact that the intuitive findings of different people, even of different experts, are often inconsistent. Thus, for example, while Prof. Ryle tells us that "voluntary" and "involuntary" in their ordinary use are applied only to actions which ought not to be done, his colleague Prof. Austin states in another connection: "... for example, take 'voluntarily' and 'involuntarily': we may join the army or make a gift voluntarily, we may hiccough or make a small gesture involuntarily..."⁵ If agreement about usage cannot be reached within so restricted a sample as the class of Oxford Professors of Philosophy, what are the prospects when the sample is enlarged?

Let us now look at the question of how to verify an assertion that a given person uses a word in a given way or with a given sense. It seems to me that, roughly speaking, there are two basic approaches, which I shall call the "extensional" and the "intensional", though any really adequate procedure will probably have to be a combination of both. In the extensional approach one observes a reasonably large class of cases in which the subject applies the word, and then one "sees" or "elicits" the meaning by finding what is common to these cases. For some reason or other this method, with all of its obvious difficulties, is thought by many people to be more scientific than the intensional approach. In the latter, one asks the subject what he means by the given word or how he uses it; the one proceeds in Socratic fashion to test this first answer by confronting the subject with counterexamples

and borderline cases, and so on until the subject settles down more or less permanently upon a definition or account. The difficulties with this method are also very considerable, and I hold no particular brief for it. I only wish to say that it has just as legitimate a claim to be "right" as the extensional method has. Things become interesting when, as will often happen, the two approaches give different results. What the analyst sees in common among the cases to which the subject applies the term may have little or nothing to do with what the subject says he means by the term; and, further, the subject may in fact apply the term to cases which lack the characteristics which he himself considers essential for proper application of the term. In whatever manner these conflicts are to be resolved, I wish only to repeat that the outcome of the intensional approach is as much a characterization of ordinary usage as the outcome of the extensional approach, assuming of course that the same subjects are involved. Thus, even if Prof. Ryle had determined that ordinary folk in fact apply the word "voluntary" only to actions which ought not to be done, while philosophers apply it to meritorious actions as well, he would be far from establishing that philosophers and ordinary folk apply the word "voluntary" in different ways, i.e., attach different senses to it.⁶ For if he had proceeded by the intensional route he might have found that both philosophers and ordinary folk tend to give the same sort of definition or account — perhaps something similar to what one finds in the dictionary — and this would have been evidence that they use the term in the *same* sense. Both approaches may lead to interesting results, but I do not see that one is scientific and the other is not, or that one is useful and the other is not.

Now the ordinary language philosophers, I believe, tend toward an armchair version of the extensional method, though sometimes they read the dictionary for intensional guidance before surveying the cases in which they know or suppose the term would be applied. This extensional approach appears in the quoted passage from Prof. Ryle,⁷ and it may also be seen in the following quotation from J. O. Urmson's article, "Some Questions Concerning Validity":

"In his popular book, *The Nature of the Physical World*, . . . Eddington said in effect that desks were not really solid. Miss Stebbing, in her book *Philosophy and the Physicists* (showed) that this way of putting things involved illegitimate mystification; this she did by simply pointing out that if one asked what we ordinarily mean by *solid* we immediately realise that we mean something like 'of the consistency of such things

as desks'. Thus she showed conclusively that the novelty of scientific theory does not consist, as had been unfortunately suggested, in showing the inappropriateness of ordinary descriptive language."⁸

I interpret (perhaps erroneously) Urmson and Stebbing to be saying that if you look and see to which objects people regularly apply the word "solid", you will find that by and large these objects have the consistency of such things as desks. But from this it does not follow that by "solid" we *mean* something like "of the consistency of such things as desks". If you want to know what we mean by "solid", you should in addition ask us; the answers will very likely be inconclusive, but they will be relevant. Perhaps they will tend to converge on several senses, one of which could be "not hollow, having its interior entirely filled with matter". If you follow out *this* strand of the ordinary use of "solid" it will no longer appear so obvious that Eddington was using "solid" in a new or mystifying sense when he said that desks are not really solid. Indeed, the effectiveness of Eddington's remark seems to derive from the fact that the properties which one is inclined to regard as the defining properties of solidity do not really belong to the objects to which we customarily apply the term "solid", and the interest of this fact is in no way diminished by repeating "Oh, but we *do* apply the word 'solid' to such things as desks". Of course we do, but we also say that by "solid" we mean "having its interior entirely filled with matter", or something like that. The collision is *within* ordinary usage, and not between it and scientific theory.

I should like now to mention some of the difficulties which attend the two methods I have discussed. Only when one gets down to cases do these problems appear with clarity, and in this general account I can list just a few. In connection with the extensional approach we have the problem of deciding which occurrences of the word are to be considered (especially when the word is thought to have different senses) and what are the relevant features of the objects to which the word is applied or of the situations in which it is applied. Much depends upon how these objects or situations are described (or "thought of") in the data from which the meaning is to be "elicited".⁹ Further, shall we describe the objects and situations in terms of the properties which they really have, or in terms of the properties which they are thought by the subject to have? (If the latter alternative is chosen, then even the extensional approach might lead to the conclusion that tables are not really solid, in the ordinary sense of "solid"). When it comes to seeing what is common to the various items which make up the extension, the

difficulties become still worse. The objects in any collection will have infinitely many properties in common; furthermore, two words with quite different senses may happen to have the same extension or, even more likely, their extensions may happen to coincide in the domain investigated. Thus perhaps Prof. Ryle happened to consider only some part of the extension of "voluntary act" which is also a part of the extension of "voluntary act that ought not to be done". On top of all these problems is the problem of determining what will justify the conclusion that a word has more than one sense or use. We shall never find a case in which it is not true that all items of the extension have *something* in common, even when on other grounds we are inclined to say that the word in question has two senses. Some principle is needed for singling out the "real" or "important" properties, but it is not easy to state what that would be. In view of all these matters it becomes apparent that the task of looking at the applications of a word and "seeing" what it means is not so simple, after all.

Before describing some of the difficulties attending the intensional approach I should like to add one more observation which pertains mainly to the extensional approach. We have all heard the wearying platitude that "you can't separate" the meaning of a word from the entire context in which it occurs, including not only the actual linguistic context but also the aims, feelings, beliefs, and hopes of the speaker, the same for the listener and any bystanders, the social situation, the physical surroundings, the historical background, the rules of the game, and so on ad infinitum. There is no doubt some truth in this, but I fail to see how it helps one get started in an empirical investigation of language. At the very least, provisional divisions of the subject have to be made somewhere. It seems to me that there is much to be said for the well-known syntax-semantics-pragmatics division, and that often many of the factors which the ordinary language philosophers find in common among the cases in which an expression is employed belong more to the pragmatics of the expression than to its semantics. In particular, most of the facts which are expressed by statements of the form "He wouldn't say that unless he..." belong in the category of the pragmatics of the expression and should be avoided when "eliciting" or "seeing" the meaning. To take an example, consider all the fuss about the sentence "I know it, but I may be wrong", which has been called everything from "contradiction" to "nonsense". Perhaps it is true that ordinarily I wouldn't say "I know it" unless I felt great confidence in what I was asserting, and it might also be true that ordinarily I wouldn't

say "I may be wrong" unless I felt only a small amount of such confidence. So that if I say "I know it, but I may be wrong" the listener may be momentarily befuddled before he hits upon the right diagnosis of the form "He wouldn't say that unless he . . .". But all this does not suffice to show that "I know it, but I may be wrong" is contradictory or nonsensical according to ordinary usage. The confidence I signify by saying "I know it" does not have to be mentioned in giving a semantical account of the word "know", but only in describing its pragmatics. Likewise, when I say "I may be wrong" I do not *imply* that I have no confidence in what I have previously asserted; I only indicate it. If I do have the confidence and yet say "I may be wrong", I have not told a falsehood, though I may indeed have misled someone. Limitation of time prevents my going further into this matter, and I only bring it up so as to be able to formulate the following: it seems to me that not only do the ordinary language philosophers tend toward an armchair version of the extensional method, but also they are inclined to overlook the semantic-pragmatic distinction when they find what is common to the situations in which a given word is used. If, in the example concerning "voluntary", Prof. Ryle means that the ordinary man applies the word only to actions of which he disapproves, while the philosopher applies it to approved actions as well, and that hence the philosopher uses the word in a stretched, extraordinary sense, then this would be an example of the sort of semantic-pragmatic confusion which I am here trying to describe.¹⁰

The intensional approach to the problem of verifying assertions about meaning or use seems to involve us in a conceptual difficulty of its own. If we are to do justice to the notions (1) that what an individual means by a word depends at least in part upon what he *wants* to mean by that word, and (2) that he may have to think awhile before he discovers what he "really" means by a given word, we are led to consider a test which will amount to a sort of Socratic questionnaire. That is, there will have to be prodding questions aimed at drawing the subject's attention to borderline cases, counterexamples, and various awkward consequences of his first and relatively off-hand answers. If as a result of these questions he is inclined to give a different answer from the one he gave at first, we may describe the phenomenon in various ways: we can say that he has changed his mind or learned something new, or we can say that he has now managed to find a better way of expressing what he really means (and perhaps has always meant) by the word. In general, it does not seem possible to differentiate in a practical way

between *finding out* what someone means by a word, and *influencing* his linguistic behavior relative to that word. A philosopher who had been brought up on Plato might be inclined to think that when a group of persons were subjected to a Socratic interrogation their answers would at first be very diverse and later would tend to converge on one, or at most a few, definitions of the term in question. It would be interesting to know whether questionnaires could be designed which would have this effect, and also whether one could design a sort of anti-Socratic questionnaire which would tend to lead subjects to greater and greater disagreement.¹¹

On the whole, it seems to me unwise, initially at least, to try to limit oneself either to the intensional or to the extensional approach. It will in practice be difficult to separate the two when one is designing a concrete test for determining the ordinary sense of a given word; they are rather to be thought of as different moments or tendencies which actual methods will combine in varying proportions. It could easily happen, however, as it has in certain well-known analogous cases, that experience would teach us to revise our conceptual framework, or at least to change our estimates of what is important and valuable.¹² It may turn out desirable to distinguish different senses of the expression "ordinary use", corresponding to different methods of verifying statements in which this expression occurs, and one would then wish to know in which, if any, of these senses it would be true and important to say that in philosophic problems words do not have their ordinary use.

NOTES

- ¹ This paper and the one which succeeds it, by Prof. Cavell, were read as parts of a symposium at a meeting of the American Philosophical Association, Pacific Coast Division, on December 19, 1957.
- ² Ryle, Gilbert, *The Concept of Mind*, New York, Barnes and Noble, 1949, pp. 69 ff.
- ³ Ryle, Gilbert, "Ordinary Language", *The Philosophical Review*, vol. LXII (1953), pp. 167 ff.
- ⁴ I do not deny that the armchair method is adequate for many purposes. Perhaps it is adequate even for deciding the correctness or incorrectness of statements like those of Prof. Ryle about "voluntary". I would not trust it, however, to decide such a question as whether the ordinary use of "inadvertently" is the same as that of "automatically" (as applied to actions). (Cf. Austin, J. L., "A Plea for Excuses", *Arist. Soc. Papers*, 1956-7, p. 24). Of course, even in these cases I do not propose to dispense with it, but only to add to it.
- ⁵ *Op. cit.*, p. 17.
- ⁶ Note Ryle's use of the word "sense" in the passage quoted.
- ⁷ i.e., he tries to show that the philosophic sense is different from the ordinary sense, using as evidence information that the philosophic extension is different from the ordinary extension.
- ⁸ Urmson, J. O., "Some Questions Concerning Validity", *Rev. Int. de Phil.*, vol. 7 (1953), pp. 217 ff.
- ⁹ e.g., in Ryle's case they could be described or taken as actions which ought not to be done, or as actions proceeding from the will or from the individual's own choice or full consent, or in any of a number of other ways.
- ¹⁰ Consider the sentence, "Jones disapproves of playing golf on Sunday". In this sentence the word "Jones" occurs directly, while the expression "playing golf on Sunday" occurs obliquely. Thus, the truth-value of the given sentence may be reversed when the expression "playing golf on Sunday" is replaced by an expression having the same denotation but different sense. Now since I use the word "property" in such a way as to satisfy Leibniz' Law (in the form: if A is the same as B, then any property of A is a property of B), I regard the given sentence as expressing a property of Jones but not as expressing a property of the act of playing golf on Sunday. Consequently I am led to hold that although the properties of an act will in general be relevant to the semantics of any expression referring to that act, the approval or disapproval of an act by someone does not constitute one of its properties.
- ¹¹ If the latter were possible, it would obviously be awkward to interpret the Socratic method as a method of *finding out* what the subject means, as against *teaching* him something new.
- ¹² Thus, the concept of "definiteness of intention", introduced by Professor Arne Naess (cf. "Toward a Theory of Interpretation and Preciseness", in Linsky, L., *Semantics and the Philosophy of Language*, pp. 256 ff.), seems to have a devastating relevance to many of the characteristic assertions of the ordinary language philosophers.

MUST WE MEAN WHAT WE SAY?¹

by

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That what we ordinarily say and mean may have a direct and deep control over what we can philosophically say and mean is an idea which many philosophers find oppressive. It might be argued that in part the oppression results from misunderstanding; that the new philosophy which proceeds from ordinary language is not *that* different from traditional methods of philosophizing, and that the frequent attacks upon it are misdirected. But I shall not attempt to be conciliatory, both because I think the new philosophy at Oxford is critically different from traditional philosophy, and because I think it is worth trying to bring out their differences as fully as possible. There *is*, after all, something oppressive about a philosophy which seems to have uncanny information about our most personal philosophical assumptions (those, for example, about whether we can ever know for certain of the existence of the external world, or of other minds; and those we make about favorite distinctions between "the descriptive and the normative", or between matters of fact and matters of language) and which inveterately nags us about them. Particularly oppressive when that philosophy seems so often *merely* to nag and to try no special answers to the questions which possess us — unless it be to suggest that we sit quietly in a room. Eventually, I suppose, we will have to look at that sense of oppression itself: such feelings can come from a truth about ourselves which we are holding off.

My hopes here are modest. I shall want to say why, in my opinion, some of the arguments Professor Mates brings against the Oxford philosophers he mentions are on the whole irrelevant to their main concerns. And this will require me to say something about what I take to be the significance of proceeding, in one's philosophizing, from what we ordinarily say and mean. That will not be an easy thing to do without

appearing alternately trivial and dogmatic. Perhaps that is only to be expected, given the depth and the intimacy of conflict between this way of proceeding in philosophy and the way I take Mates to be following. These ways of philosophy seem, like friends who have quarreled, to be able neither to tolerate nor to ignore one another. I shall frequently be saying something one could not fail to know; and that will appear trivial. I shall also be suggesting that something we know is being overemphasized and something else not taken seriously enough; and that will appear dogmatic. But since I am committed to this dialogue, the time is past for worrying about appearances.

Professor Mates is less concerned to dispute specific results of the Oxford philosophers than he is to question the procedures which have led these philosophers to claim them. In particular, he doubts that they have assembled the sort of evidence which their "statements about ordinary language" require. As a basis for his skepticism, Mates produces a disagreement between two major figures of the school over the interpretation of an expression of ordinary language — a disagreement which he regards as symptomatic of the shallowness of their methods.² On Mates' account of it, the conflict is not likely to be settled successfully by further discussion. We are faced with two professors (of philosophy, it happens) each arguing (claiming, rather) that the way he talks is the right way and that what he intuits about language is the truth about it. But if this is what their claims amount to, it hardly seems worth a philosopher's time to try to collect evidence for them.

To evaluate the disagreement between Austin and Ryle, we may distinguish among the statements they make about ordinary language, three types:³ (1) There are statements which produce *instances* of what is said in a language ("We do say . . . but we don't say —"; "We ask whether . . . but we do not ask whether —"); (2) Sometimes these instances are accompanied by *explications*—statements which make explicit what is implied when we say what statements of the first type instance us as saying ("When we say . . . we imply (suggest, say) —"; "We don't say . . . unless we mean —"). Such statements are checked by reference to statements of the first type. (3) Finally, there are *generalizations*, to be tested by reference to statements of the first two types. Since there is no special problem here about the testing of generalizations, we will be concerned primarily with the justification of statements of the first two types, and especially with the second.

Even without attempting to be more precise about these differences, the nature of the clash between Ryle and Austin becomes somewhat clearer. Notice, first of all, that the statement Mates quotes from Austin is of the first type: "Take 'voluntarily' . . . : we may . . . make a gift voluntarily . . ." — which I take to be material mode for, "We say, 'The gift was made voluntarily' ". (The significance of this shift of "mode" will be discussed.) Only one of the many statements Mates quotes from Ryle is of this type, *viz.*, "It makes sense . . . to ask whether a boy was responsible for breaking a window, but not whether he was responsible for finishing his homework in good time . . .". The statements of Ryle's which clash with Austin's are different: "In their most ordinary employment 'voluntary' and 'involuntary' are used . . . as adjectives applying to actions which ought not to be done. We discuss whether someone's action was voluntary or not only when the action seems to have been his fault . . . etc." These do not produce *instances* of what we say (the way "We say, 'The boy was responsible for breaking the window' " does); they are generalizations — as the phrases "actions which" and "only when" show — to be tested by producing such instances.

It is true that the instance quoted from Austin does go counter to Ryle's generalization: making a gift is not always something which ought not to be done, or something which is always someone's fault. There is clearly a clash here. But is our only intelligent course *at this point* to take a poll? Would it be dogmatic or unempirical of us to conclude simply that Ryle is wrong about this, that he has settled upon a generalization to which an obvious counter instance has been produced? It is, moreover, an instance which Ryle himself may well be expected to acknowledge as counter to his generalization; indeed, one which he might have produced for himself. The fact that he did not need indicate only that he was too quick to accept a generalization, not that he is without (good) evidence for it. One of Mates' objections to Ryle can be put this way: Ryle *is* without evidence — anyway, without very good evidence — because he is not entitled to a statement of the first type (one which presents an *instance* of what we say) in the absence of experimental studies which demonstrate its occurrence in the language.

To see that this objection, taken in the general sense in which Mates urges it, is groundless, we must bear in mind the fact that these statements — statements that something is said in English — are being made by native speakers of English. Such speakers do not, in *general*, need evidence for what is said in the language; they are the source of such

evidence. It is from them that the descriptive linguist takes the corpus of utterances on the basis of which he will construct a grammar of that language. To answer *some* kinds of specific questions, we will have to engage in that "laborious questioning" Mates insists upon, and count noses; but in general, to tell what is and isn't English, and to tell whether what is said is properly used, the native speaker can rely on his own nose; if not, there would be nothing to count. No one speaker will say everything, so it may be profitable to seek out others; and sometimes you (as a native speaker) may be unsure that a form of utterance is as you say it is, or is used as you say it is used, and in that case you will have to check with another native speaker. And because attending so hard to what you say may itself make you unsure more often than is normal, it is a good policy to check more often. A good policy, but not a methodological necessity. The philosopher who proceeds from ordinary language, in his use of himself as subject in his collection of data, may be more informal than the descriptive linguist (though not more than the linguistic theorist using examples from his native speech); but there is nothing in that to make the data, in some general way, suspect.

Nor does this imply a reliance on that "intuition or memory" which Mates (p. 165) finds so objectionable. In claiming to know, in general, whether we do or do not use a given expression, I am not claiming to have an infallible memory for what we say, any more than I am claiming to remember the hour when I tell you what time we have dinner on Sundays. A normal person may forget and remember certain words, or what certain words mean, in his native language, but (assuming that he has used it continuously) he does not remember the *language*. There is a world of difference between a person who speaks a language natively and one who knows the language fairly well. If I lived in Munich and knew German fairly well, I might try to intuit or guess what the German expression for a particular phenomenon is. Or I might ask my landlady; and that would probably be the extent of the laborious questioning the problem demanded. Nor does the making of either of the sorts of statement about ordinary language I have distinguished rely on a claim that "[we have] already amassed . . . a tremendous amount of empirical information about the use of [our] native language" (Mates, *ibid.*) That would be true if we were, say, making statements about the history of the language, or about its sound system, or about the housewife's understanding of political slogans, or about a special form in the morphology of some dialect. But for a native speaker to say what, in ordinary circumstances, is said when, no such special information is needed or claimed.

All that is needed is the truth of the proposition that a natural language is what native speakers of that language speak.

Ryle's generalization, however, requires more than simple, first level statements of instances; it also requires statements of the second type, those which contain first level statements together with an "explication" of them. When Ryle claims that "... we raise questions of responsibility only when someone is charged, justly or unjustly, with an offence", he is claiming both, "We say 'The boy was responsible for breaking a window', but we do not say 'The boy was responsible for finishing his homework in good time'", and also claiming, "When we say 'The boy was responsible for (some action)' we imply that the action was an offence, one that ought not to have been done, one that was his fault". I want to argue that Ryle is, in general, as entitled to statements of this second type as he is to statements of the first type; although it is just here that the particular generalization in question misses. We know Austin's example counters Ryle's claim because we know that the statement (of the second type), "When we say, 'The gift was made voluntarily' we imply that the action of making the gift was one which ought not to be done, or was someone's fault" is false. This is clearly knowledge which Mates was relying on when he produced the clash between them. I will take up statements of the second type in a moment.

Before proceeding to that, let us look at that clash a bit longer: its importance has altered considerably. What Austin says does not go fully counter to Ryle's story. It is fundamental to Austin's account to emphasize that we cannot *always* say of actions that they were voluntary, even when they obviously were not involuntary either. Although we can (sometimes) say, "The gift was made voluntarily", it is specifically not something we can say about ordinary, unremarkable cases of making gifts. Only when the action (or circumstances) of making the gift is in some way unusual (instead of his usual Christmas bottle, you give the neighborhood policeman a check for \$1000), or extraordinary (you leave your heirs penniless and bequeath your house to your cat), or untoward (you give your rocking horse to your new friend, but the next morning you cry to have it back), can the question whether it was voluntary intelligibly arise. Ryle has not completely neglected this: his "actions which ought not be done" and his "action [which] seems to have been ... [someone's] fault" are clearly examples of actions which are abnormal, untoward, questionable; so he is right in saying that about these we (sometimes) raise the question whether they were voluntary.

His error lies in characterizing these actions incompletely, and in wrongly characterizing those about which the question *cannot* arise. Normally, it is true, the question whether satisfactory, correct or admirable performances are voluntary does not arise; but this is because there is usually nothing about such actions to question; nothing has gone wrong.

Not seeing that the condition for applying the term "voluntary" holds quite generally — *viz.*, the condition that there be something (real or imagined) fishy about any performance intelligibly so characterized — Ryle construes the condition too narrowly, supposes that there must be something *morally* fishy about the performance. He had indeed sensed trouble where trouble was: the philosophical use of "voluntary" stretches the idea of volition out of shape, beyond recognition. And his diagnosis of the trouble was sound: philosophers imagine, because of a distorted picture of the mind, that the term "voluntary" must apply to all actions which are not involuntary (or unintentional), whereas it is only applicable where there is some specific reason to raise the question. The fact that Ryle fails to specify its applicability precisely enough no more vitiates his entire enterprise than does the fact that he indulges a mild form of the same vice he describes: he frees himself of the philosophical tic of stretching what is true of definite segments of what we do to cover *everything* we do (as epistemologists stretch doubt to cover everything we say), but not from the habit of identifying linguistic antitheses with logical contradictories:⁴ in particular, he takes the question, "Voluntary or not?" to mean, "Voluntary or involuntary?", and seems to suppose that (responsible) actions which are not contemptible must be admirable, and that whatever I (responsibly) do either is my fault or else is to my credit. These antitheses miss exactly those actions about which the question "Voluntary or not?" really has no sense, *viz.*, those ordinary, unremarkable, natural things we do which make up most of our conduct and which are neither admirable nor contemptible; which, indeed, could only erroneously be said to go on, in general, in *any* special way.⁵ Lacking firmness here, it is not surprising that Ryle's treatment leaves the subject a bit wobbly. Feeling how *enormously* wrong it is to remove "voluntary" from a *specific* function, he fails to sense the slighter error of his own specification.⁶

I have said that the ordinary language philosopher is also and equally entitled to statements of the second type I distinguished, which means that he is entitled not merely to say what (words) we say, but equally

to say what we should mean in (by) saying them. Let us turn to statements of this type and ask what the relation is between what you explicitly say and what you imply; or, to avoid begging the question, ask how we are to account for the fact (supposing it to be a fact) that we only say or ask A ("X is voluntary", or "Is X voluntary?") where B is the case (something is, or seems, fishy about X).⁷ The philosophical problem about this arises in the following way: Philosophers who proceed from ordinary language are likely to insist that if you say A where B is not the case, you will be misusing A, or distorting its meaning. But another philosopher will not want to allow that, because it makes the relation between A and B appear to be a logical one (If A then B; and if not-B then not-A); whereas logical relations hold only between statements, not between a statement and the world: *that* relation is "merely" conventional (or, even, causal?). So the occasion on which we (happen to?) *use* a statement cannot be considered part of its meaning or logic. The solution is then to call the latter the semantics of the expression and the former its pragmatics.

But if we can forget for a moment that the relation between A and B *cannot* be a logical one, we may come to feel how unpalatable it is to say that it is not logical; or rather, to say that nothing *follows* about B from the utterance of A. It is unpalatable because we do not accept a question like "Did you do that voluntarily?" as appropriate about any and every action. If a person asks you whether you dress the way you do voluntarily, you will not understand him to be curious merely about your psychological processes (whether your wearing them "proceeds from free choice..."); you will understand him to be implying or suggesting that your manner of dress is in some way peculiar. If it be replied to this that "voluntary" does not *mean* "peculiar" (or "special" or "fishy") and hence that the implication or suggestion is part merely of the pragmatics of the expression, not part of its *meaning* (semantics), my rejoinder is this: that reply is relevant to a different claim from the one urged here; it is worth saying *here* only if you are able to account for the *relation* between the pragmatics and the semantics of the expression. In the absence of such an account, the reply is empty. For consider: If we use Mates' formula for computing the pragmatic value of an expression — "He wouldn't say that unless he..." — then in the described situation we will complete it with something like, "... unless he thought that my way of dressing is peculiar". Call this implication of the utterance "pragmatic"; the fact remains that he wouldn't (couldn't) say what he did without implying what he did: he **MUST MEAN** that

my clothes are peculiar. I am less interested now in the "mean" than I am in the "must". (After all, there is bound to be some reason why a number of philosophers are tempted to call a relation logical; "must" is logical.) But on this, the "pragmatic" formula throws no light whatever.

What this shows is that the formula does not help us account for the element of necessity ("must") in statements whose implication we understand. But it is equally unhelpful in trying to explain the implication of a statement whose use we do *not* understand (the context in which the formula enters Mates' discussion). Imagine that I am sitting in my counting house counting up my money. Someone who knows that I do that at this hour every day passes by and says, "You ought to do that". What should we say about his statement? That he does not know what "ought" means (what the dictionary says)? That he does not know how to use the word? That he does not know what obligation is? Applying the formula, we compute: "He wouldn't say that unless he asks himself whenever he sees anyone doing anything, 'Ought that person to be doing that or ought he not?' ". This may indeed account for his otherwise puzzling remark; but it does so by telling us something we did not know about *him*; it tells us nothing whatever we did not know about the words he used. Here it is *because* we know the meaning and use of "ought" that we are forced to account in the way Mates suggests for its extraordinary occurrence. I take Mates' formula, then, to be expandable into: "Since I understand the meaning and use of his expression, he wouldn't say that unless he . . .". Perhaps Mates would consider this a distortion and take a different expansion to be appropriate: "He wouldn't say that unless he was using his words in a special way". But now "say that" has a very different force. The expanded form now means, "I know what his expression would ordinarily be used to say, but he can't wish to say that: I don't understand what he is saying". In neither of its expansions, then, does the formula throw any light on the way an expression is being used: in the one case we already know, in the other we have yet to learn. (Another expansion may be: "He wouldn't say that unless he was using X to mean Y". But here again, it is the semantics and pragmatics of Y which are relevant to understanding what is said, and the formula presupposes that we already understand Y.)

Our alternatives seem to be these: Either (1) we deny that there is any rational (logical, grammatical) constraint over the "pragmatic implications" of what we say — or perhaps deny that there *are* any impli-

cations, on the ground that the relation in question is not deductive — so that unless what I say is flatly false or unless I explicitly contradict myself, it is pointless to suggest that what I say is wrong or that I must mean something other than I say; or else (2) we admit the constraint and say either (a) since all necessity is logical, the “pragmatic implications” of our utterance are (quasi-)logical implications; with or without adding (b) since the “pragmatic implications” cannot be construed in terms of deductive (or inductive) logic, there must be some “third sort” of logic; or we say (c) some necessity it not logical. None of these alternatives is without its obscurities, but they are clear enough for us to see that Mates is taking alternative (1),⁸ whereas the philosopher who proceeds from ordinary language is likely to feel the need of some form of (2). Alternative (2a) brings out part of the reason behind the Oxford philosopher’s insistence that he is talking logic, while (2b) makes explicit the reason other philosophers are perplexed at that claim.⁹

The difference between alternatives (1) and (2) is fundamental; so fundamental, that it is very difficult to argue. When Mates says, “Perhaps it is true that ordinarily I wouldn’t say ‘I know it’ unless I felt great confidence in what I was asserting . . .”, what he says is not, if you like, *strictly* wrong; but it is wrong — or, what it implies is wrong. It implies that whether I confine the formula “I know . . .” to statements about which I feel great confidence is *up to me* (*rightly* up to me); so that if I say “I know . . .” in the absence of confidence, I have not misused language, and in particular I have not stretched the *meaning* of the word “know”. And yet, if a child were to say “I know . . .” when you know the child does not *know* (is in no position to say he knows) you may reply, “You don’t really mean (nb) you *know*, you only mean you believe”; or you may say, “You oughtn’t to say you *know* when you only *think* so”.

There are occasions on which it would be useful to have the “semantic-pragmatic” distinction at hand. If, for example, a philosopher tells me that the statement, “You ought to do so-and-so” expresses private emotion and is hortatory and hence not, strictly speaking, meaningful, then it may be worth replying that nothing follows about the meaning (semantics) of a statement from the way it is used (pragmatics); and this reply may spare our having to make up special brands of meaning. But the time for that argument is, presumably, past.¹⁰ What needs to be argued now is that something *does* follow from the fact that a term is used in its usual way: it entitles you (or, using the term, you entitle

others) to make certain inferences, draw certain conclusions. (This is part of what you say when you say that you are talking about the *logic* of ordinary language.) *Learning what these implications are is part of learning the language*; no less a part than learning its syntax, or learning what it is to which terms apply: they are an essential part of what we communicate when we talk. Intimate understanding is understanding which is implicit. Nor *could* everything we say (mean to communicate), in normal communication, be said explicitly¹¹ — otherwise the only threat to communication would be acoustical. We are, therefore, exactly as responsible for the specific implications of our utterances as we are for their explicit factual claims. And there can no more be some general procedure for securing that what one implies is appropriate than there can be for determining that what one says is true. Misnaming and misdescribing are not the only mistakes we can make in talking. Nor is lying its only immorality.

I am prepared to conclude that the philosopher who proceeds from ordinary language is entitled, without special empirical investigation, to assertions of the second sort we distinguished, *viz.*, assertions like, "We do not say 'I know . . . ' unless we mean that we have great confidence . . .", and like "When we ask whether an action is voluntary we imply that the action is fishy" (call this S). But I do not think that I have *shown* that he is entitled to them, because I have not shown what kind of assertions they are; I have not shown when such assertions should be said, and by whom, and what should be meant in saying them. It is worth trying to indicate certain complexities of the assertions, because they are easy to overlook. Something important will be learned if we realize that we do *not* know what kind of assertion S is.

When (if) you feel that S is necessarily true, that it is *a priori*, you will have to explain how a statement which is obviously not analytic *can* be true *a priori*. (That S is not analytic is what (is all) that is shown by Mates' arguments about the "semantic-pragmatic" confusion; it is perfectly true that "voluntary" does not *mean* (you will not find set beside it in a dictionary) "fishy".) When I am impressed with the necessity of statements like S, I am tempted to say that they are categorical — about the concept of an action *überhaupt*. (A normal action is neither voluntary nor involuntary, neither careful nor careless, neither expected nor unexpected, neither right nor wrong...) This would account for our feeling of their necessity: they are instances (not of

Formal, but) of Transcendental Logic. But this is really no explanation until we make clearer the need for the concept of an action in general.

However difficult it is to make out a case for the necessity of S, it is important that the temptation to call it *a priori* not be ignored; otherwise we will acquiesce in calling it synthetic, which would be badly misleading. Misleading (wrong) because we know what would count as a disproof of statements which are synthetic (to indicate the willingness to entertain such disproof is the point of calling a statement synthetic), but it is not clear what would count as a disproof of S. The feeling that S must be synthetic comes, of course, partly from the fact that it obviously is not (likely to be taken as) analytic. But it comes also from the ease with which S may be mistaken for the statement, "Is X voluntary?" implies that X is fishy" (T), which does seem obviously synthetic. But S and T, though they are true together and false together, are not everywhere interchangeable; the identical state of affairs is described by both, but a person who may be entitled to say T, may not be entitled to say S. Only a native speaker of English is entitled to the statement S, whereas a linguist describing English may, though he is not a native speaker of English, be entitled to T. What entitles him to T is his having gathered a certain amount and kind of evidence in its favor. But the person entitled to S is not entitled to *that* statement for the same reason. He *needs* no evidence for it. It would be misleading to say that he *has* evidence for S, for that would suggest that he has done the sort of investigation the linguist has done, only less systematically, and this would make it seem that his claim to know S is very weakly based. And it would be equally misleading to say that he does *not* have evidence for S, because that would make it appear that there is something he still needs, and suggests that he is not yet entitled to S. But there is nothing he needs, and there is no evidence (which it makes sense, in *general*, to say) he has: the question of evidence is irrelevant.

An examination of what does entitle a person to the statement S would be required in any full account of such statements. Such an examination is out of the question here. But since I will want to claim that Mates' "two methods" for gathering evidence in support of "statements about ordinary language" like S are irrelevant to what entitles a person to S, and since this obviously rests on the claim that the concept of evidence is, in general, irrelevant to them altogether, let me say just this: The clue to understanding the sort of statement S is lies in appreciating the fact that "we", while plural, is first person. First person *singular* forms have recently come in for a great deal of attention, and they have been

shown to have very significant logical-epistemological properties. The plural form has similar, and equally significant, properties; but it has been, so far as I know, neglected.¹² The claim that in general we do not require evidence for statements in the first person plural present indicative, does not rest upon a claim that we cannot be wrong about what we are doing or about what we say, but only that it would be extraordinary if we were (often). My point about such statements, then, is that they are sensibly questioned only where there is some special reason for supposing what I say about what I (we) say to be wrong; only here is the request for evidence competent. If I am wrong about what he does (they do), that may be no great surprise; but if I am wrong about what I (we) do, that is liable, where it is not comic, to be tragic.

Statements like T have their own complexities, and it would be unwise even of them to say simply that they are synthetic. Let us take another of Mates' examples: "I know it' is not (ordinarily) said unless the speaker has great confidence in it" (T'). Mates takes this as patently synthetic, a statement about matters of fact (and there is no necessary connection among matters of fact). And so it might be, said by a Scandinavian linguist as part of his description of English. But if that linguist, or if a native speaker (i.e., a speaker entitled to say, "We do not say 'I know it' unless...") uses T' in teaching someone to speak English, or to remind a native speaker of something he knows but is not bearing in mind, T' sounds less like a descriptive statement than like a rule.

Because of what seems to be the widespread idea that rules always sort with commands and must therefore be represented as imperatives, this complementarity of rule and statement may come as something of a shock. But that such complementarity exists can be seen in writings which set out the rules for games or ceremonies or languages. In *Hoyle's Rules of Games* we find statements like, "The opponent at declarer's left makes the opening lead... Declarer's partner then lays his whole hand face up on the table, with his trumps if any on the right. The hand so exposed is the *dummy*. ... The object of play is solely to win tricks, in order to fulfil or defeat the contract"; in Robert's Rules of Order, the rules take the form, "The privileged motion to adjourn takes precedence of all others, except the privileged motion 'to fix the time to which to adjourn', to which it yields" (in Section 17, headed "To Adjourn"); taking a grammar at random we find, "Mute stems form the nominative singular by the addition of -s in the case of masculines and feminines... Before -s of the nominative singular, a labial mute (p, b) remains unchanged." These are all statements in the indicative, not the imperative,

mood. (Some expressions in each of these books tell us what we *must* do; others that we *may*. I will suggest later a reason for this shift.) In one light, they appear to be descriptions; in another to be rules. Why should this be so? What is its significance?

The explanation of the complementarity has to do with the fact that its topic is actions. When we say how an action is done (how to act) what we say may report or describe the way we *in fact* do it (if we are entitled to say how "we" do it, i.e., to say what we do, or say what we say) but it may also lay out a way of doing or saying something which is to be *followed*. Whether remarks like T' — remarks "about" ordinary language, and equally about ordinary actions — are statements or rules depends upon how they are taken: if they are taken to state facts and are supposed to be believed, they are statements; if they are taken as guides and supposed to be followed, they are rules. Such expressions are no more "in themselves" rules or (synthetic) statements than other expressions are, in themselves, postulates or conclusions or definitions or replies. We might put the relation between the two contexts of T' this way: Statements which describe a language (or a game or an institution) are rules (are binding) if you want to speak that language (play that game, accept that institution); or, rather, *when* you are speaking that language, playing that game, etc. *If it is TRUE to say "I know it" is not used unless you have great confidence in it*, then, *when you are speaking English, it is WRONG (a misure) to say "I know it" unless you have great confidence in it*. Now the philosopher who proceeds from ordinary language assumes that he and his interlocutors are speaking from within the language, so that the question of whether you want to speak that language is pointless. Worse than pointless, because strictly the ordinary language philosopher does not, in general, *assume* that he and his interlocutors are speaking from within a given (their native) language — any more than they speak their native language, in general, *intentionally*. The only condition relevant to such philosophizing is that you speak (not this or that language, but) period.

At this point the argument has become aporetic. "Statements about ordinary language" like S, T and T' are not analytic, *and* they are not (it would be misleading to call them) synthetic (just like that).¹³ Nor do we know whether to say they are *a priori*, or whether to account for their air of necessity as a dialectical illusion, due more to the motion of our argument than to their own nature. Given our current alternatives, there is no way to classify such statements; we do not yet know what they are.

Before searching for new ways into these problems, I should perhaps justify my very heavy reliance on the idea of *context*, because on Mates' description of what a statement of context involves, it should be impossible ever to make one. Let me recall his remarks: "We have all heard the wearying platitude that 'you can't separate' the meaning of a word from the entire context in which it occurs, including not only the actual linguistic context, but also the aims, feelings, beliefs, and hopes of the speaker, the same for the listener and any bystanders, the social situation, the physical surroundings, the historical background, the rules of the game, and so on *ad infinitum*" (p. 168). Isn't this another of those apostrophes to the infinite which prevents philosophers from getting down to cases?¹⁴ Of course if I have to go on about the context of "voluntary" *ad infinitum*, I would not get very far with it. But I would claim to have characterized the context sufficiently (for the purpose at hand) by the statement that something is, or is supposed to be, fishy about the action. Giving directions for using a word is no more prodigious and unending a task than giving directions for anything else. The context in which I make a martini with vodka is no less complex than the context in which I make a statement with "voluntary". Say, if you like, that these actions take place in infinitely complex contexts; but then remember that you can be given directions for doing either. It may be wearying always to be asked for a story within which a puzzling remark can seriously be imagined to function; but I know no better way of maintaining that relevance, or sense of reality, which each philosopher claims for himself and claims to find lacking in another philosophy. At least it would spare us the surrealism of worries like, "'What time is it?' asserts nothing, and hence is neither true nor false; yet we all know what it means *well enough* to answer it";¹⁵ or like, "If we told a person to close the door, and received the reply, 'Prove it!' should we not, to speak mildly, grow somewhat impatient?"¹⁶

In recommending that we ignore context in order to make "provisional divisions" of a subject and get an investigation started, Mates is recommending the wrong thing for the right reason. It is true that we cannot say everything at once and that for some problems some distinction of the sort Mates has in mind may be of service. My discontent with it is that it has come to deflect investigation — I mean from questions on which Oxford philosophy trains itself. Where your concern is one of constructing artificial languages, you may explain that you mean to be considering only the syntax (and perhaps semantics) of a language, and not its pragmatics. Or where it becomes important to emphasize a distinc-

tion between (where there has come to be a distinction between) scientific and metaphysical assertion, or between factual report and moral rule, you may set out a "theory" of scientific or factual utterance. In these cases you will be restricting concern in order to deal with certain properties of formal systems, certain problems of meaning, and to defeat certain forms of nonsense. Flat contradiction, metaphysical assertion masquerading as scientific hypothesis, mere whim under the posture of an ethical or esthetic (or psychological or legal) judgment — these perhaps need hounding out. But the philosopher who proceeds from ordinary language is concerned less to avenge sensational crimes against the intellect than to redress its civil wrongs; to steady any imbalance, the tiniest usurpation, in the mind. This inevitably requires reintroducing ideas which have become tyrannical (e.g., existence, obligation, certainty, identity, reality, truth . . .) into the specific contexts in which they function naturally. This is not a question of cutting big ideas down to size, but of giving them the exact space in which they can move without corrupting. Nor does our wish to rehabilitate rather than to deny or expel such ideas (by such sentences as, "We can never know for certain . . ."; "The table is not real (really solid)"; "To tell me what I ought to do is always to tell me what you want me to do" . . .) come from a sentimental altruism. It is a question of selfpreservation: for who is it that the philosopher punishes when it is the mind itself which assaults the mind?

I want now to turn two other, related, questions on which Mates finds himself at issue with the Oxford philosophers. The first concerns their tendency to introduce statements of the first sort I distinguished not with "We do say . . ." but with "We *can* say . . ." and "We *can't* say . . .". The second question concerns, at last directly, reasons for saying that we "must" mean by our words what those words *ordinarily* mean.

Let me begin by fulfilling my promise to expand upon my remark that Austin's saying, "We may make a gift voluntarily" is "material mode" for "We can say, 'The gift was made voluntarily'". The shift from talking about language to talking about the world occurs almost imperceptibly in the statement of Austin's which Mates quotes — almost as though he thought it did not much matter *which* he talked about. Let me recall the passage from Austin: "...take 'voluntarily' and 'involuntarily': we may join the army or make a gift voluntarily, we

may hiccup or make a small gesture involuntarily." He begins here by mentioning a pair of words, and goes on to tell us what we may in fact do. With what right? Why is it assumed that we find out what voluntary and involuntary actions *are* (and equally, of course, what inadvertent and automatic and pious, etc., actions are) by asking when we should *say* of an action that it is voluntary or inadvertent or pious, etc.?

But what is troubling about this? If you feel that finding out what something is must entail investigation of the world rather than of language, perhaps you are imagining a situation like finding out what somebody's name and address are, or what the contents of a will or a bottle are, or whether frogs eat butterflies. But now imagine that you are in your armchair reading a book of reminiscences and come across the word "umiak". You reach for your dictionary and look it up. Now what did you do? Find out what "umiak" means, or find out what an umiak is? But how could we have discovered something about the world by hunting in the dictionary? If this seems surprising, perhaps it is because we forget that we learn language and learn the world *together*, that they become elaborated and distorted together, and in the same places. We may also be forgetting how elaborate a process the learning is. We tend to take what a native speaker does when he looks up a noun in a dictionary as the characteristic process of learning language. (As, in what has become a less forgivable tendency, we take naming as the fundamental source of meaning.) But it is merely the end point in the process of learning the word. When we turned to the dictionary for "umiak" we already knew everything about the word, as it were, but its combination: we knew what a noun is and how to name an object and how to look up a word and what boats are and what an Eskimo is. We were all prepared for that umiak. What seemed like finding the world in a dictionary was really a case of bringing the world to the dictionary. We had the world with us all the time, in that armchair; but we felt the weight of it only when we felt a lack in it. Sometimes we will need to bring the dictionary to the world. That will happen when (say) we run across a small boat in Alaska of a sort we have never seen and wonder — what? What it is, or what it is called? In either case, the learning is a question of aligning language and the world.¹⁷ What you need to learn will depend on what specifically it is you want to know; and how you can find out will depend specifically on what you already command. How we answer the question, "What is X?" will depend, therefore, on the specific case of ignorance and of knowledge.

It sometimes happens that we know everything there is to know about a situation — what all of the words in question mean, what all of the relevant facts are; and everything is in front of our eyes. And yet we feel we don't know something, don't understand something. In this situation, the question "What is X?" is very puzzling, in exactly the way philosophy is very puzzling. We feel we want to ask the question, and yet we feel we already have the answer. (One might say we have all the *elements* of an answer.) Socrates says that in such a situation we need to remind ourselves of something. So does the philosopher who proceeds from ordinary language: we need to remind ourselves of *what we should say when*.¹⁸ But what is the point of reminding ourselves of that? When the philosopher asks, "What should we say here?", what is meant is, "What would be the normal thing to say here?", or perhaps, "What is the most natural thing we could say here?" And the point of the question is this: answering it is sometimes the only way to tell — tell others and tell for ourselves — what the situation *is*.

Sometimes the only way to tell. But when? The nature of the Oxford philosopher's question, and the nature of his conception of philosophy, can be brought out if we turn the question upon itself, and thus remind ourselves of when it is we need to remind ourselves of what we should say when. Our question then becomes: When should we ask ourselves when we should (and should not) say "The x is F" in order to find out what an $F(x)$ is? (For "The x is F" read "The action is voluntary (or pious)", or "The Statement is vague (or false)", or "The question is misleading".) The answer suggested is, when you have to. When you have more facts than you know how to make of, or when you do not know what new facts would show. When, that is, you need a clear view of what you already know. When you need to do philosophy.¹⁹ Euthyphro does not need to learn any new facts, yet he needs to learn something: you can say either that in the *Euthyphro* Socrates was finding out what "piety" means or finding out what piety is.

When the philosopher who proceeds from ordinary language tells us, "You can't say such-and-such", what he means is that you cannot say that *here* and communicate *this* situation to others, or understand it for yourself.²⁰ This is sometimes what he means by calling certain expressions "misuses" of language, and also makes clear the consequences of such expressions: they break our understanding. The normativeness

which Mates felt, and which is certainly present, does not lie in the ordinary language philosopher's assertions *about* ordinary use; what is normative is exactly ordinary use itself.

The way philosophers have practiced with the word "normative" in recent years seems to me lamentable. But it is too late to avoid the word, so even though we cannot now embark on a diagnosis of the ills which caused its current use, or those which it has produced, it may be worth forewarning ourselves against the confusions most likely to distract us. The main confusions about the problem of "normativeness" I want to mention here are these: (1) the idea that descriptive utterances are opposed to normative utterances; and (2) that prescriptive utterances are (typical) instances of normative utterances.

We have touched upon these ideas in talking about rule-statement complementarity; here we touch them at a different point. In saying here that it is a confusion to speak of some general opposition between descriptive and normative utterances, I am not thinking primarily of the plain fact that rules have counterpart (descriptive) statements, but rather of the significance of that fact, *viz.*, that what such statements describe are *actions* (and not, e.g., the *movements* of bodies, animate or inanimate). The most characteristic fact about actions is that they can — in various specific ways — go wrong, that they can be performed incorrectly. This is not, in any restricted sense, a moral assertion, though it points the moral of intelligent activity. And it is as true of describing as it is of calculating or of promising or plotting or warning or asserting or defining . . . These are actions which we perform, and our successful performance of them depends upon our adopting and following the ways in which the action in question is done, upon what is normative for it. Descriptive statements, then, are not opposed to ones which are normative, but in fact presuppose them: we could not do the thing we call describing if language did not provide (we had not been taught) ways normative for describing.

The other point I wish to emphasize is this: if a normative utterance is one used to create or institute rules or standards, then prescriptive utterances are not examples of normative utterances. Establishing a norm is not telling us how we *ought* to perform an action, but telling us how the action *is* done, or how it is *to be* done.²¹ Contrariwise, telling us what we ought to do is not instituting a norm to cover the case, but rather presupposes the existence of such a norm, i.e., presupposes that there is something to do which it would be correct to do here. Telling us what we ought to do may involve *appeal* to a pre-existent rule or stan-

dard, but it cannot constitute the establishment of that rule or standard. We may expect the retort here that it is just the *appeal* which is the sensitive normative spot, for what we are really doing when we appeal to a rule or standard is telling somebody that they ought to adhere to it. Perhaps this will be followed by the query, And suppose they don't accept the rule or standard to which you appeal, what then? The retort is simply false. And to the query one may reply that this will not be the first time we have been tactless; nor can we, to avoid overstepping the bounds of relationship, follow every statement by, "... if you accept the facts and the logic I do", nor every evaluation by "... if you accept the standards I do". Such cautions will finally suggest appending to everything we say "... if you mean by your words what I mean by mine". Here the pantomime of caution concludes. It is true that we sometimes appeal to standards which our interlocutor does not accept; but this does not in the least show that what we are there really doing is attempting to institute a standard (of our own). Nor does it in the least show that we are (merely) expressing our own opinion or feeling on the matter. We of course *may* express our private opinion or feeling — we normally do so where it is not clear what (or that any) rule or standard fits the case at hand and where we are therefore not willing or able to appeal to any.

The practice of appealing to a norm can be abused, as can any other of our practices. Sometimes people appeal to a rule when we deserved more intimate attention from them. Just as sometimes people tell us what we ought to do when all they mean is that they want us to. But this is as much an abuse where the context is moral as it is where the context is musical ("You ought to accent the appoggiatura"), or scientific ("You ought to use a control group here"), or athletic ("You ought to save your wind on the first two laps"). Private persuasion (or personal appeal) is not the paradigm of ethical utterance, but represents the breakdown (or the transcending) of moral interaction. We can, too obviously, become morally inaccessible to one another; but to tell us that these are the moments which really constitute the moral life will only add confusion to pain.

If not, then, by saying what actions *ought* to be performed, how *do* we establish (or justify or modify or drop) rules or standards? What general answer can there be to this general question other than, In various ways, depending on the context? Philosophers who have imagined that the question has one answer for all cases must be trying to assimilate the members of Football Commissions, of Child Development Research

Teams, of University Committees on Entrance Requirements, of Bar Association Committees to Alter Legal Procedures, of Departments of Agriculture, of Bureaus of Standards, and of Essene Sects, all to one "sort" of person, doing one "sort" of thing, *viz.*, establishing (or changing) rules and standards. Whereas the fact is that there are, in each case, different ways normative for accomplishing the particular normative tasks in question. It has in recent years been emphasized past acknowledgment that even justifications require justification. What now needs emphasizing is that (successfully) justifying a statement or an action is not (cannot be) justifying its justification.²² The assumption that the appeal to a rule or standard is only justified where that rule or standard is simultaneously established or justified can only serve to make such appeal seem hypocritical (or anyway shaky) and the attempts at such establishment or justification seem tyrannical (or anyway arbitrary).

It would be important to understand why we have been able to overlook the complementarity of rule and statement and to be content always to sort rules with imperatives. Part of the reason for this comes from a philosophically inadequate (not to say disastrous) conception of action; but this inadequacy itself will demand an elaborate accounting. There is another sort of reason for our assumption that what is binding upon us must be an imperative; one which has to do with our familiar sense of alienation from established systems of morality, perhaps accompanied by a sense of distance from God. Kant tells us that a perfectly rational being does in fact (necessarily) conform to "the supreme principle of morality", but that we imperfectly rational creatures are necessitated *by* it, so that for us it is (always appears as) an imperative. But if I understand the difference Kant sees here, it is one *within* the conduct of rational animals. So far as Kant is talking about (the logic of) action, his Categorical Imperative can be put as a Categorical Declarative (description-rule), i.e., a description of what it *is* to act morally: When we (you) act morally, we act in a way we would regard as justified universally, justified no matter who had done it. (This categorical formulation does not tell us how to determine *what was done*; neither does Kant's categorical formulation, although, by speaking of "the" maxim of an action, it pretends to, or anyway makes it seem less problematical than it is.) Perhaps it is by now a little clearer why we are tempted to retort, "But suppose I don't *want* to be moral?"; and also why it would be irrelevant here. The Categorical Declarative does not tell you what you *ought* to do *if* you want to be moral (and hence is untouched by the feeling that no imperative can really be *categorical*, can bind us no

matter what); it tells you (part of) what you in fact do when you *are* moral. It cannot — nothing a philosopher says can — insure that you will not act immorally; but it is entirely unaffected by what you do or do not want.

I am not saying that rules do not sometimes sort with imperatives, but only denying that they always do. In the Britannica article (11th edition) on chess, only one paragraph of the twenty or so which describe the game is headed "Rules", and only here are we told what we *must* do. This paragraph deals with such matters as the convention of saying "j'adoube" when you touch a piece to straighten it. Is the difference between matters of this kind and the matter of how pieces move, a difference between penalties (which are imposed for misplay) and moves (which are accepted in order to play at all) — so that we would cheerfully say that we can play (are playing) chess without the "j'adoube" convention, but less cheerfully that we can play without following the rule that "the Queen moves in any direction, square or diagonal, whether forward or backward"? This would suggest that we may think of the difference between rule and imperative as one between those actions (or "parts" of actions) which are easy (natural, normal) for us, and those we have to be encouraged to do. (What I do as a rule you may have to be made or directed to do.) We are likely to forget to say "j'adoube", so we have to be *made* (to remember) to do it; but we do not have to be *made* to move the Queen in straight, unobstructed paths.²³ This further suggests that what is thought of as "alienation" is something which occurs *within* moral systems; since these are profoundly haphazard accumulations, it is no surprise that we feel part of some regions of the system and feel apart from other regions.²⁴

So the subject of responsibility, of obligation and commitment, opens into the set of questions having to do with differences between doing a thing wrongly or badly (strangely, ineptly, inexactly, partially...) and not doing the thing at all. These differences take us into a further region of the concept of an action: we have noted that there are many (specific) ways in which an action can go wrong (at least as many as the myriad excuses we are entitled to proffer when what we have done has resulted in some unhappiness); but it would be incorrect to suppose that we are *obligated* to see to it (to take precautions to insure), *whenever* we undertake to do anything, that none of these ways will come to pass. Our obligation is to avoid doing something at a time and place or in a way which is *likely* to result in some misfortune, or to avoid being careless where it is easy to be, or to be *especially* careful where the

action is dangerous or delicate, or avoid the temptation to skip a necessary step when it seems in the moment to make little difference. If for *all* excuses there were relevant obligations, then there would be no excuses and action would become intolerable. Any *particular* excuse may be countered with a *specific* obligation; not even the best excuse will always get you off the hook (That is no excuse; you should have known that was likely to result in an accident, you ought to have paid *particular* heed here, etc.).

Without pretending to give an account of (this part of) obligation, what I think the foregoing considerations indicate is this: a statement of what we *must* do (or say) has point only in the context (against the background) of knowledge that we are in fact doing (or saying) a thing, but doing (saying) it — or running a definite risk of doing or saying it — badly, inappropriately, thoughtlessly, tactlessly, self-defeatingly, etc.; or against the background of knowledge that we are in a certain position or occupy a certain office or station, and are *behaving* or *conducting ourselves* inappropriately, thoughtlessly, self-defeatingly . . . The same is true of statements about what we *may* do, as well as those containing other “modal auxiliaries” — e.g., about what we *should* do, or what we *are* or *have* to do, or are *supposed* to do, and about one sense of what we *can* do; these are all intelligible only against the background of what we are doing or are in a position (one sense of “able”) to do. These “link verbs” share the linguistic peculiarity that while they are verb-like forms they cannot stand as the main verb of a sentence. This itself would suggest that their use is not one of prescribing some new action to us, but of setting an action which is antecedently relevant to what we are doing or to what we are — setting it relevantly into the larger context of what we are doing or of what we are.²⁵ “You must (are supposed, obliged, required to) move the Queen in straight paths. . .” or, “You may (can, are allowed or permitted to) move the Queen in straight paths . . .” say (assert) no more than “You (do, in fact, always) move the Queen in straight paths . . .”; which of them you say on a given occasion depends not on any special motive or design of yours, nor upon any special mode of argument. There is no question of *going from* “is” to “must”, but only of appreciating which of them should be said when, i.e., of appreciating the position or circumstances of the person to whom you are speaking. Whatever makes one of the statements true makes them all true, though not all appropriate.

To tell me what I must do is not the same as to tell me what I ought to do. I must move the Queen in straight paths (in case I am absent-

minded and continue moving it like the Damsel, cf. n. 23). What would it mean to tell me that I *ought* to move the Queen in straight paths? "Ought", unlike "must", implies that there is an alternative; "ought" implies that you can, if you choose, do otherwise. This does *not* mean merely that there is something else to do which is in your *power* ("I *can* move the Queen like the Knight; just watch!") but that there is one within your *rights*. But if I say truly and appropriately, "You must . . ." then in a perfectly good sense nothing you then do can prove me wrong. You *CAN* *push the little object called the Queen* in many ways, as you can *lift* it or *throw* it across the room; not all of these will be *moving the Queen*. You *CAN* ask, "Was your action voluntary" and say to yourself, "All I mean to ask is whether he had a sensation of effort just before he moved", but that will not be finding out whether the action was voluntary. Again, if I have borrowed money then I *must* (under normal circumstances) pay it back (even though it is rather painful).²⁶ It makes sense to tell me I *ought* to pay it back only if there is a specific reason to suppose, say, that the person from whom I got the money *meant to give* it to me rather than merely *lend* it (nevertheless he needs it badly, worse than I know), or if there is a reason to pay it back tomorrow instead of next week, when the debt falls due (I'll save interest; I'll only spend it and have to make another loan). The difference here resembles that between doing a thing and doing the thing well (thoughtfully, tactfully, sensibly, graciously . . .).

This difference may be made clearer by considering one way principles differ from rules. Rules tell you what to do when you do the thing at all; principles tell you how to do the thing well, with skill or understanding. In competitive games, acting well amounts to doing the sort of thing that will win, so the principles of games recommend strategy. "No raise should [nb] be given to partner's suit without at least Q-x-x, J-10-x, K-x-x, A-x-x, or any four trumps . . .". But you could fail to adopt this and still play bridge, even play well. It is a principle strategy in Culbertson's system;²⁷ but another expert may have a different understanding of the game and develop principles of strategy which are equally successful. Principles go with understanding. (Having an understanding of a game is not knowing the rules; you might find a book called Principles of Economics or Psychology, but none called Rules of Economics, etc.) Understanding a principle involves knowing how and where to apply it. But some moves seem so immediately to be called for by the principles of strategy, that their formulations come to be thought of as rules: should we say, "The third hand plays high . . ."

or "The third hand should play high . . ."? You may, strictly speaking, be playing bridge if you flout this, but you won't be doing the sort of thing which will win (and therefore not really playing? When is not doing a thing well not really doing the thing?). All players employ maxims (which may be thought of as formulating strategies as though they were moves) in order to facilitate their play; like everything habitual or summary, maxims have their advantages and their dangers. Both the rules which constitute playing the game, and the "rules" or maxims which contribute to playing the game well have their analogues in ordinary moral conduct.

I think it is sometimes felt that drawing an analogy between moral conduct and games makes moral conduct seem misleadingly simple (or trivial?), because there are no rules in moral conduct corresponding to the rules about how the Queen moves in chess.²⁸ But this misses the point of the analogy, which is that moves and actions have to be done *correctly*; not just any movement you make will be a move, or a promise, a payment, a request. This does not mean that promising *is* (just) following rules. Yet if someone is tempted not to fulfill a promise, you may say, "Promises are kept", or "We keep our promises (that is the sort of thing a promise is)", thus employing a rule-description — what I have called a categorial declarative. You may say, "You must keep this promise" (you are underestimating its importance; last time you forgot). This is not the same as, "You ought to keep this promise", which is only sensible where you have a reason for breaking it strong enough to allow you to do so without blame (there is a real alternative), but where you are being enjoined to make a *special* effort or sacrifice. (This is partly why "You ought to keep promises" is so queer. It suggests that we not only always want badly to get out of fulfilling promises, but that we always have some good (anyway, *prima facie*) reason for not keeping them (perhaps our own severe discomfort) and that therefore we are acting *well* when we do fulfill. But we aren't, normally; neither well nor ill.) "Ought" is like "must" in requiring a background of action or position into which the action in question is set; and, like "must", it does not form a command, a pure imperative. All of which shows the hopelessness of speaking, in a *general* way, about the "normativeness" of expressions. The Britannica "rules" tell us what we *must* do *in playing* chess, not what we ought to do *if* we want to play. You (must) mean (imply), in speaking English, that something about an action is fishy when you say, "The action is voluntary"; you (must) mean, when you ask a person, "Ought you to do that?" that there is some

specific way in which what he is doing might be done more tactfully, carefully, etc. . . . Are these imperatives? Are they categorical or hypothetical? Have you in no way contradicted yourself if you flout them? (cf. n. 26.)

That "modal imperatives" ("must", "supposed to", "are to", "have to" . . .) require the recognition of a background action or position into which the relevant action is placed, indicates a portentous difference between these forms of expression and pure imperatives, commands. Whether I can command depends only upon whether I have power or authority, and the only characteristics I must recognize in the object of the command are those which tell me that the object is subject to my power or authority. Employing a modal "imperative", however, requires that I recognize the object as a *person* (someone doing something or in a certain position) to whose reasonableness (reason) I appeal in using the second person. (Compare, "Open, Sesame!" with "You must open, Sesame.") This is one reason that commands, pure imperatives, are not paradigms of moral utterance, but represent an alternative to such utterance.

Without pretending that my argument for it has been nearly full or clear enough, let me, by way of summary, flatly state what it is I have tried to argue about the relation between what you say and what you (must) mean, i.e., between what you (explicitly) say and what saying it implies or suggests: If "what A (an utterance) means" is to be understood in terms of (or even as directly related to) "what is (must be) meant in (by) saying A",²⁹ then the meaning of A will not be given by its analytic or definitional equivalents, or by its deductive implications. Intension is not a substitute for intention. Although we would not call the statement "When we say we know something we imply (mean) that we have confidence, that we are in a position to say we know . . ." analytic, yet if the statement is true it is necessarily true in just this sense: if it is true, then when you ask what the statement supposes you to ask you (must) mean what the statement says you (must) mean. Necessary and not analytic: it was — apart from the parody of Kant — to summarize, and partly explain, this peculiarity that I called such statements categorial declaratives: declarative, because something is (authoritatively) made known; categorial, because in telling us what we (must) mean by asserting that (or questioning whether) x is F, they tell us what it is for an x to be F (an action to be moral, a statement claiming knowledge to be a statement expressing knowledge, a move-

ment to be a move).³⁰ Shall we say that such statements formulate the rules or the principles of grammar — the moves or the strategies of talking? (And is this, perhaps, to be thought of as a difference between grammar and rhetoric? But becoming clearer about this will require us to see more clearly the difference between not doing a thing well (here, saying something) and not doing the thing; and between doing a thing badly and not doing the thing.) The significance of categorial declaratives lies in their teaching or reminding us that the “pragmatic implications” of our utterances are (or, if we are feeling perverse, or tempted to speak carelessly, or chafing under an effort of honesty, let us say *must be*) meant; that they are an essential part of what we mean when we say something, of what it is to mean something. And what we mean (intend) to say, like what we mean (intend) to do, is something we are responsible for.

Even with this slight rehabilitation of the notion of normativeness, we can begin to see the special sense in which the philosopher who proceeds from ordinary language is “establishing a norm” in employing his second type of statement. He is certainly not *instituting* norms, nor is he *ascertaining* norms (see note 21); but he may be thought of as *confirming* or *proving* the existence of norms when he reports or describes how we (how to) talk, i.e., when he says (in statements of the second type) what is normative for utterances instanced by statements of the first type. Confirming and proving are other regions of establishing. I have suggested that there are ways normative for instituting and for ascertaining norms; and so are there for confirming or proving or reporting them, i.e., for employing locutions like, “We can say . . .”, or “When we say . . . we imply —”. The swift use made of them by the philosopher serves to remind mature speakers of a language of something they know; but they would erroneously be employed in trying to report a special usage of one’s own, and (not unrelated to this) could not be used to change the meaning of an expression. Since saying something is never *merely* saying something, but is saying something with a certain tune and at a proper cue and while executing the appropriate business, the sounded utterance is only a salience of what is going on when we talk (or the unsounded when we think); so a statement of “what we say” will give us only a feature of what we need to remember. But a native speaker will normally know the rest: learning it was part of learning the language.

Let me warn against two tempting ways to avoid the significance of this: (1) It is perfectly true that English might have developed differ-

ently than it has and therefore have imposed different categories on the world than it does; and if so, it would have enabled us to assert, describe, question, define, promise, appeal, etc., in ways other than we do. But using English now — to converse with others in the language, or to understand the world, or to think by ourselves — means knowing which forms in what contexts are normative for performing the activities we perform by using the language. (2) It is no escape to say: "Still I can say what I like; I needn't always use normal forms in saying what I say; I can speak in extraordinary ways, and you will perfectly well understand me." What this calls attention to is the fact that language provides us with ways for (contains forms which are normative for) speaking in special ways, e.g., for changing the meaning of a word, or for speaking, *on particular occasions*, loosely or personally, or paradoxically, cryptically, metaphorically . . . Do you wish to claim that you can speak strangely yet intelligibly — and this of course means intelligibly to yourself as well — in ways not provided in the language for speaking strangely?

It may be felt that I have not yet touched one of Mates' fundamental criticisms. Suppose you grant all that has been said about an ordinary use being normative for what anyone says. Will you still wish to ask: "Does it follow that the ordinary uses which are normative for what professors say are the same as the ordinary uses which are normative for what butchers and bakers say?" Or perhaps: "Is an ordinary use for a professor an *ordinary* ordinary use?" Is that a sensible question?

To determine whether it is, we must appreciate what it is to talk together. The philosopher, understandably, often takes the isolated man bent silently over a book as his model for what using language is. But the primary fact of natural language is that it is something spoken, spoken together. Talking together is acting together, not making motions and noises at one another, nor transferring unspeakable messages or essences from the inside of one closed chamber to the inside of another. The difficulties of talking together are, rather, *real* ones: the activities we engage in by talking are intricate and intricately related to one another. I suppose it will be granted that the professor and the baker can talk together. Consider the most obvious complexities of cooperative activity in which they engage: there is commenting ("Nice day"); commending, persuading, recommending, enumerating, comparing ("The pumpernickle is good, but the whole wheat and the rye even better");

grading, choosing, pointing ("I'll have the darker loaf there"); counting, making change, thanking; warning ("Careful of the step"); promising ("Be back next week") . . . ; all this in addition to the whole nest or combination of actions which comprise the machinery of talking: asserting, referring, conjoining, denying, . . . Now it may be clearer why I wish to say: if the professors and the baker did not understand each other, the professors would not understand one another either.

You may still want to ask: "Does this mean that the professor and baker use particular words like 'voluntary' and 'involuntary', or 'inadvertently' and 'automatically' the same way? The baker may never have used these words at all." But the question has *now* become, since it is about *specific* expressions, straightforwardly empirical. Here Mates' "two methods" (pp 165 ff.) at last become relevant. But I am at the moment less interested in determining what empirical methods would be appropriate to investigate the matter than I am in posing the following questions: What should we say if it turned out, as it certainly might, that they in fact do use the words differently? Should we, for example, say that therefore we never have a right to say that people use words in the same way without undertaking an empirical investigation; or perhaps say that therefore they speak different languages? What should make us say that they do not speak the same language? Do we really know what it would be like to embark upon an empirical investigation of the *general* question whether we (ordinarily, ever) use language the way other people do?

There is too much here to try to unravel. But here are some of the threads: The words "inadvertently" and "automatically", however recondite, are ordinary; there are ordinary contexts (nontechnical, nonpolitical, nonphilosophical contexts) which are normative for their use. It may be that half the speakers of English do not know (or cannot say, which is not the same) what these contexts are. Some native speakers may even use them interchangeably. Suppose the baker is able to convince us that he does. Should we then say: "So the professor has no right to say how *"we* use" "inadvertently", or to say that "when *we* use the one word we say something different from what we say when we use the other"?" Before accepting that conclusion, I should hope that the following consideration would be taken seriously: When "inadvertently" and "automatically" seem to be used indifferently in recounting what someone did, this may not at all show that they are being used synonymously, but only that what each of them says is separately true of the person's action. The decanter is broken and you did it. You may

say (and it may be important to consider that you are already embarrassed and flustered) either: "I did it inadvertently" or "I did it automatically". Are you saying the same thing? Well, you automatically *grabbed the cigarette* which had fallen on the table, and inadvertently *knocked over the decanter*. Naming actions is a sensitive occupation.³¹ It is easy to overlook the distinction because the two adverbs often go together in describing actions in which a sudden movement results in some mishap.

Suppose the baker does not accept this explanation, but replies: "I use 'automatically' and 'inadvertently' in exactly the same way. I could just as well have said: 'I grabbed the cigarette inadvertently and knocked over the decanter automatically'". Don't we feel the temptation to reply: "You may *say* this, but you can't say it and describe the same situation; you can't mean what you would mean if you said the other". But suppose the baker insists he can? Will we then be prepared to say: "Well you can't say the one and mean what *I* mean by the other"? Great care would be needed in claiming this, for it may look like I am saying, "I know what I mean and I say they are different". But why is the baker not entitled to this argument? What I must not say is: "I know what words mean in *my* language". Here the argument would have pushed me to madness. It *may* turn out (depending upon just what the dialogue has been and where it was stopped) that we should say to the baker: "If you cooked the way you talk, you would forgo special implements for different jobs, and peel, core, scrape, slice, carve, chop and saw, all with one knife. The distinction is there, in the language (as implements are there to be had), and you just impoverish what you can say by neglecting it. And there is something you aren't noticing about the world".³²

But to a philosopher who refuses to acknowledge the distinction we should say something more: not merely that he impoverishes what he can say about actions, but that he is a poor theorist of what it is to do something. The philosopher who asks about everything we do, "Voluntary or not?" has a poor view of action (as the philosopher who asks of everything we say, "True or false?" or "Analytic or synthetic?" has a poor view of communication), in something like the way a man who asks the cook about every piece of food, "Was it cut or not?" has a poor view of preparing food. The cook with only one knife is in much better condition than the philosopher with only "Voluntary or involuntary?" to use in dividing actions, or "True or false?" to use in hacking out meaningful statements. The cook can get on with the preparation of the meal even if he must improvise a method here and there, and

makes more of a mess than he would with more appropriate implements. But the philosopher can scarcely *begin* to do his work; there is no job the philosopher has to get on with; nothing ulterior he must do with actions (e.g., explain or predict them), or with statements (e.g., verify them). What he wants to know is what they are, what it is to do something and to say something. To the extent that he improvises a way of getting past the description and division of an action or a statement, or leaves a mess in his account — to that extent he laves his own job undone. If the philosopher is trying to get clear about what preparing a meal is and asks the cook, “Do you cut the apple or not?”, the cook may say, “Watch me!”, and then core and peel it. “Watch me!” is what we should reply to the philosopher who asks of our normal, ordinary actions, “Voluntary or not?” and who asks of our ethical and esthetic judgments, “True or false?” Few speakers of a language utilize the full range of perception which the language provides, just as they do without so much of the rest of their cultural heritage. Not even the philosopher will come to possess all of his past, but to neglect it deliberately is foolhardy. The consequence of such neglect is that our philosophical memory and perception become fixated upon a few accidents of intellectual history.

I have suggested that the question of “[verifying] an assertion that a given person uses a *word* in a given way or with a given sense” (Mates, *ibid*, my emphasis) is not the same as verifying assertions that “We say . . .” or that “When we say . . . we imply —”. This means that I do not take the “two basic approaches” which Mates offers in the latter part of his paper to be directed to the same question as the one represented in the title he gives to his paper (at least on my interpretation of that question). The questions are designed to elicit different types of information; they are relevant (have point) at different junctures of investigation. Sometimes a question is settled by asking others (or ourselves) what we say here, or whether we ever say such-and-such; on the basis of these data we can make statements like, “‘Voluntary’ is used of an action only where there is something (real or imagined) fishy about it”. I take this to be a “statement about ordinary language” (and equally, about voluntary action). But surely it is not, under ordinary circumstances, an assertion about how a word is used by *me* (or “some given person”); it is a statement about how the word is used in English. Questions about how a given person is using some *word* can sensibly

arise only where there is some specific reason to suppose that he is using the word in an unusual way. This point can be put the other way around: the statement "I (or some given person) use (used) the word X in such-and-such a way" implies (depending on the situation) that you intend (intended) to be using it in a special way, or that someone else is unthinkingly misusing it, or using it misleadingly, and so on. This is another instance of the principle that actions which are normal will not tolerate any special description. In a *particular* case you may realize that words are not to be taken normally, that some want or fear or special intention of the speaker is causing an aberration in the drift of his words. A little girl who says to her brother, "You can have half my candy" may mean, "Don't take any!"; the husband who screams in fury, "Still no buttons!" may really be saying, "If I were honest, I'd do what Gaugin did". A knave or a critic or an heiress may say, "X is good" and mean "I want or expect or command you to like (or approve of) X"; and we, even without a special burden of malice, or of taste, or of money, may sometimes find ourselves imitating them.

Mates interprets Ryle's assertion that the ordinary use of "voluntary" applies to actions which are disapproved to mean that "the ordinary man applies the word only to actions of which he disapproves" (p. 169); this apparently involves a reference to that man's personal "aims, feelings, beliefs, and hopes"; and these, in turn, are supposedly part merely of the pragmatics (not the semantics) of a word. It is therefore a mistake, Mates concludes, to claim that the philosopher is using the word in a "stretched, extraordinary sense" (*ibid*), my emphasis) merely on the ground that he may not happen to feel disapproving about an action he calls voluntary. The mistake, however, is to suppose that the ordinary use of a word is a function of the internal state of the speaker. (It is sometimes to emphasize that your remarks about "use" are not remarks about such states that you want to say you are talking about the *logic* of ordinary language.) Another reason for the tenacity of the idea that a statement of what we mean when we say so-and-so (a statement of the second type) must be synthetic is that we suppose it to be *describing* the mental processes of the person talking. To gain perspective on that idea, it may be of help to consider that instead of saying to the child who said he *knew* (when we knew he had no right to say so), "You mean you *think* so", we might have said, "You *don't* know (or, That is not what it is to know something); you just think so". This says neither more nor less than the formulation about what he *means*, and neither of them is a description of what is going on inside the child.

They are both statements which teach him what he has a *right* to say, what knowledge is.

Mates tells us (ibid) that his "intensional approach" is meant, in part, "to do justice to the notions (1) that what an individual means by a word depends at least in part upon what he wants to mean by that word, and (2) that he may have to think awhile before he discovers what he 'really' means by a given word". With respect to the first notion, I should urge that we do justice to the fact that an individual's intentions or wishes can no more produce the general meaning for a word than they can produce horses for beggars, or home runs from pop flies, or successful poems out of unsuccessful poems.³³ This may be made clearer by noticing, with respect to the second notion, that often when an individual is thinking "what he 'really' means" (in the sense of having second thoughts about something), he is not thinking what he really means by a given *word*. You have second thoughts in such cases just because you cannot make words mean what you wish (*by wishing*); it is for that reason that what you say on a given occasion may not be what you really mean. To say what you really mean you will have to say something different, change the words; or, as a special case of this, change the meaning of a word. Changing the meaning is not wishing it were different. This is further confirmed by comparing the locutions, "X means YZ" and "I mean by X, YZ". The former holds or fails to hold, whatever I wish to mean. And the latter, where meaning does depend on me, is performative;³⁴ something I am doing to the word X, not something I am wishing about it.

What these remarks come to is this: there is no such activity as my-finding-out-what-I-mean-by-a-word. But there obviously is finding-out-what-a-word-means. You do this by consulting a dictionary or a native speaker who happens to know. There is also something we may call finding-out-what-a-word-really--means. This is done when you already know what the dictionary can teach you; when, for some reason or other, you are forced into philosophizing. Then you begin by recollecting the various things we should say were such-and-such the case. Socrates gets his antagonists to withdraw their definitions not because they do not know what their words mean, but because they do know what they (their words) mean, and therefore know that Socrates has led them into paradox. (How could I be led into a paradox if I could mean what I wished by my words? Because I must be consistent? But how could I *be inconsistent* if words would mean what I wanted them to mean?) What they had not realized was what they were saying, or, what they

were *really* saying, and so not known *what they meant*. To this extent, they had not known themselves, and not known the world. I mean, of course, the ordinary world. That may not be all there is, but it is important enough: morality is in that world, and so are force and love; so is art and a part of knowledge (the part which is about that world); and so is religion (wherever God is). Some mathematics and science, no doubt, are not. This is why you will not find out what "number" or "neurosis" or "mass" or "mass society" mean if you only listen for our ordinary uses of these terms.³⁵ But you will never find out what voluntary action is if you fail to see when we should say of an action that it is voluntary.

One may still feel the need to say "Some actions *are* voluntary and some are involuntary. It would be convenient [for what?] to call all actions voluntary which are not involuntary. Surely I can call them anything I like? Surely what I *call* them doesn't affect what they *are*?" Now: how will you tell me what "they" are?³⁶ What we need to ask ourselves here is: In what sort of situations does it make no difference what I call a thing? or: At what point in a dialogue does it become natural or proper for me to say, "I (you) can call it what I (you) like"? At this point it may be safe to say that the question is (has become) verbal.³⁷ If you really have a way of telling just what is denoted by "all actions which are not involuntary", then you can call them anything you like.

I just tried to characterize the situation in which we ordinarily ask, "What does X mean?", and to characterize the *different* situation in which we ask, "What does X really mean?" These questions neither conflict nor substitute for one another, though philosophers often take the second as a profound version of the first — perhaps to console themselves for their lack of progress. Isn't this part of the trouble about Synonymy? "Does X *really* mean the same as Y?" is not a profound version of "Does X mean the same as Y?" It (its occasion) is, though related to the first in obvious and devious ways, different. The same goes for the pair: "What did he do?" and "What did he really (literally) *do*?"; and for the pair: "What do you see?" and "What do you *really* (immediately) *see*?"; and for the pair: "Is the table solid?" and "Is the table *really* (absolutely) solid?" Since the members of the pairs are *obviously* different, philosophers who do not see that the difference in the second members lies in their occasions, in where and when they are

posed, handsomely provide special entities, new worlds, for them to be about. But this can only perpetrate — it will not penetrate — a new reality.

The profoundest as well as the most superficial questions can be understood only when they have been placed in their natural environments. (What makes a statement or a question profound is not its placing but its timing.) The philosopher is no more magically equipped to remove a question from its natural environment than he is to remove himself from any of the conditions of intelligible discourse. Or rather, he may remove himself, but his mind will not follow. This, I hope it is clear, does not mean that the philosopher will not eventually come to distinctions, and use words to mark them, at places and in ways which depart from the currently ordinary lines of thought.³⁸ But it does suggest that (and why) when his recommendations come too fast, with too little attention to the particular problem for which we have gone to him, we feel that instead of thoughtful advice we have been handed a form letter. Attention to the details of cases as they arise may not provide a quick path to an all-embracing system; but at least it promises genuine instead of spurious clarity.

Some philosophers will find this program too confining. Philosophy, they will feel, was not always in such straits; and it will be difficult for them to believe that the world and the mind have so terribly altered that philosophy must relinquish old exitements to science and to poetry. There, it may be claimed, new uses are still invented by profession, and while this makes the scientist and the poet harder to understand initially, it enables them eventually to renew and to deepen and to articulate our understanding. No wonder the philosopher will gape at such band wagons. But he must sit still. Both because, where he does not wish to invent (hopes not to invent), he is not entitled to the rewards and licenses of those who do; and because he would otherwise be running from his peculiar task — one which has become homelier perhaps, but still quite indispensable to the mind. The "unwelcome consequences" (Mates, p. 164) which may attend using words in ways which are (have become) privately extraordinary is just that our understanding should lose its grasp. Not only is it true that this can happen without our being aware of it, it is often very difficult to become aware of it — like becoming aware that we have grown pedantic or childish or slow. The meaning of words *will*, of course, stretch and shrink, and they will be stretched and be shrunk. One of the great responsibilities of the philosopher lies in appreciating the natural and the normative ways in which

such things happen, so that he may make us aware of the one and capable of evaluating the other. It is a wonderful step toward understanding the abutment of language and the world when we see it to be a matter of convention. But this idea, like every other, endangers as it releases the imagination. For some will then suppose that a private meaning is not more arbitrary than one arrived at publicly, and that since language inevitably changes, there is no reason not to change it arbitrarily. Here we need to remind ourselves that ordinary language is natural language, and that its changing is natural. (It is unfortunate that artificial language has come to seem a general *alternative* to natural language;³⁹ it would, I suggest, be better thought of as one of its capacities.) Some philosophers, apparently, suppose that because natural language is "constantly" changing it is too unstable to support one exact thought, let alone a clear philosophy. But this Heraclitean anxiety is unnecessary: linguistic change is itself an object of respectable study. And it misses the significance of that change. It is exactly because the language which contains a culture changes with the changes of that culture that philosophical awareness of ordinary language is illuminating; it is that which explains how the language we traverse every day can contain undiscovered treasure. To see that ordinary language is natural is to see that (perhaps even see why) it is normative for what can be said. And also to see how it is by searching definitions that Socrates can coax the mind down from self-assertion — subjective assertion and private definition — and lead it back, through the community, home. That this also renews and deepens and articulates our understanding tells us something about the mind, and provides the consolation of philosophers.

Professor Mates, at one point in his paper, puts his doubts about the significance of the claims of ordinary language this way: "Surely the point is not merely that if you use the word 'voluntary' just as the philosopher does, you may find yourself entangled in the philosophic problem of the Freedom of the Will" (p. 164). Perhaps the reason he thinks this is a negligible consequence is that he hears it on analogy with the assertion, "If you use the term 'space-time' just as the physicist does, you may find yourself entangled in the philosophic problem of simultaneity". The implication is that the problem must simply be faced, not avoided. I, however, hear the remark differently: If you use alcohol just as the alcoholic does, or pleasure as the neurotic does, you may find yourself entangled in the practical problem of the freedom of the will.

NOTES

- ¹ This is a later, greatly expanded, version of the paper read as part of the symposium mentioned in Mates' first note. Since writing the relevant portions of this paper, I have seen three articles which make points or employ arguments similar to those I am concerned with: R. M. Hare, "Are Discoveries About the Uses of Words Empirical?", *Journal of Philosophy*, November 1957; G. E. M. Anscombe, "On Brute Facts", *Analysis*, January, 1958; S. Hampshire and H. L. A. Hart, "Decision, Intention and Certainty", *Mind*, January, 1958. But it would have lengthened an already lengthy paper to have tried to bring out more specifically than will be obvious to anyone reading them their relevance to what I have said.
- ² I am too conscious of differences in the practices of Oxford philosophers to be happy about referring, in this general way, to a school. But nothing in my remarks depends on the existence of such a school – beyond the fact that certain problems are common to the philosophers mentioned, and that similar questions enter into their attempts to deal with them. It is with these questions (I mean, of course, with what I understand them to be) that I am concerned.
- ³ Perhaps I should say "ideal" types. The statements do not come labeled in the discourse of such philosophers, but I am going to have to trust that my placing of statements into these types will not seem to distort them.
- ⁴ The harmfulness of this habit is brought out in Austin's "A Plea for Excuses" (Proceedings of the Aristotelian Society, 1956–7, pp. 16–19). Pp. 7–12 of this paper contains an elaborate defense of (anyway Austin's version of) "ordinary language philosophy". No one concerned with the general subject of the present symposium (or, in particular, with the possibility of budging the subject of moral philosophy) should (= will) neglect its study.
- ⁵ Austin's discovery (for our time and place, anyway) of normal action is, I think, important enough to bear the philosophical weight he puts upon it – holding the clue to the riddle of Freedom. (Cf. "Excuses", p. 6.) A case can also be made out – as I try to do in a paper I hope soon to publish – that it was failure to recognize such action which produced some of the notorious paradoxes of classical Utilitarianism: what neither the Utilitarians nor their critics seem to have seen clearly and constantly is that about unquestionable (normal, natural) action no question is (can be) raised; in particular not the question whether the action ought or ought not to have been done. The point is a logical one: to raise a question about an action is to put the action in question. It is partly the failure to appreciate this which makes the classical moralists (appear?) so moralistic, allows them to suppose that the moral question is *always* appropriate (except, of course, where the action is unfree (caused?)). But this is no better than the assumption that the moral question is *never* appropriate (because we are never *really* free). Such mechanical moralism has got all the punishment it deserves in the recent mechanical antimoralism, which it must have helped inspire.
- ⁶ At the same time, Ryle leaves "involuntary" as stretched as ever when he allows himself to speak of "the involuntariness of [someone's] late arrival" (Concept of Mind, p. 72).

- ⁷ I realize that the point is controversial and that in putting so much emphasis on it I may be doing some injustice to the point of view I am trying to defend. There may be considerations which would lead one to be more temperate in making the point; but against the point of view Mates is adopting, it seems to me to demand all the attention it can get.
- ⁸ As is most clearly shown where he says (p. 169), "...When I say 'I may be wrong' I do not *imply* that I have no confidence in what I have previously asserted; I only indicate it". Why "only"? Were he willing to say "...but I do (inevitably) indicate it", there may be no argument.
- ⁹ Alternative (2b) has been taken – for different, but not unrelated, reasons – in the writings of John Wisdom (e.g., "Gods", in *Logic and Language*, edited by A. G. N. Flew (Oxford, 1951), p. 196), and in S. Toulmin's *The Place of Reason in Ethics* (Cambridge, 1950), p. 83, and in S. Hampshire's, "Fallacies in Moral Philosophy" (*Mind*, October, 1949), pp. 470–471.
- ¹⁰ It was essentially the argument with which the pragmatists attempted to subdue emotive "meaning". See John Dewey's "Ethical Subject-Matter and Language" (*Journal of Philosophy*, 1945, pp. 701–12).
- ¹¹ I think of this as a law of communication; but it would be important and instructive to look for apparent counter instances. When *couldn't* what is said be misunderstood? My suggestion is, only when nothing is implied, i.e., when everything you say is said explicitly. (Should we add, or when all of the implications of what is asserted can be made explicit *in a certain way*, e.g., by the methods of formal logic? It may be along such lines that utterances in logical form come to seem the ideal of understandable utterances, that here you can communicate *only* what you say, or else *more* than you say without endangering understanding. But we might think of formal logic not as the guarantor of understanding but as a substitute for it. (Cp. W. V. O. Quine, "Mr. Strawson on Logical Theory", *Mind*, October, 1953, pp. 444–45). Then we can express this "law of communication" this way: What needs understanding can be misunderstood.) But when *is* everything said explicitly? When the statement is about sense-data rather than the "physical" objects? When it is about the (physical) movements I make rather than the (nonphysical?) actions I perform? Perhaps the opponents of the Quest for Certainty (whose passion seems to have atrophied into a fear of the word "certain") have embarked upon a Quest for Explicitness. Strawson's notion of *pre-supposing* is relevant here, since explicitness and presupposition vary inversely. See his "On Referring", *Mind*, 1950; reprinted in *Essays in Conceptual Analysis*, edited by A. Flew (London, 1956).
- ¹² An examination of such forms is part of a more extended study on which I am now working.
- ¹³ If it still seems that statements like S and T *must* be synthetic, perhaps it will help to realize that anyway they are not *just some more* synthetic statements about voluntary action, on a par with a statement to the effect that somebody does (indeed) dress the way he does voluntarily. It may be true that if the world were different *enough*, the statements would be false; but that amounts to saying that if "voluntary" meant something other than it does, the statements would not mean what they do – which is not surprising. The statements in question are more closely related to such a statement as, "The future will resemble the past": this is not a (not just another) prediction, on a par with statements about whether it will rain. Russell's chicken

(who was fed every day throughout its life but ultimately had its neck wrung) was so well fed that he neglected to consider what was happening to other chickens. Even if he had considered this, he would doubtless still have had his neck wrung; but at least he wouldn't have been outsmarted. He could have avoided *that* indignity because he was wrong only about one thing; as Russell very properly says, "...in spite of frequent repetitions there sometimes is a failure at the last" (*Problems of Philosophy*, p. 102, original edition). But if the future were not (in the *general* sense needed) "like" the past, this would not be *a* failure. The future may wring our minds, but by that very act it would have given up trying to outsmart us.

- ¹⁴ A complaint Austin voiced in the course of his William James Lectures, on Performatives, at Harvard in the Spring term of 1955.
- ¹⁵ John Hospers, *Introduction to Philosophical Analysis*, p. 69. My emphasis.
- ¹⁶ Charles Stevenson, *Ethics and Language*, p. 26.
- ¹⁷ For modern instruction in the complexities of this question, see Austin's and P. F. Strawson's contributions to the symposium, "Truth" (*Proceedings of the Aristotelian Society*, Supplementary Vol. XXIV (1950)); D. Pears, "Universals", and "Incompatibilities of Colors" (both in *Logic and Language*, second series, edited by A. G. N. Flew (Oxford, 1953); W. V. O. Quine, "Two Dogmas of Empiricism" (*Philosophical Review*, 1951; reprinted in *From a Logical Point of View*, Harvard University Press, 1953); John Wisdom, papers collected in *Philosophy and Psycho-Analysis* (Oxford, 1953), especially "Philosophical Perplexity", "Metaphysics and Verification", and "Philosophy, Metaphysics and Psycho-Analysis".
- ¹⁸ The emphasized formula is Austin's ("Excuses", p. 7). Notice that the "should" cannot simply be replaced by "ought to", nor yet, I believe, simply replaced by "would". It will not, that is, yield its secrets to the question, "Descriptive or normative?"
- ¹⁹ This is part of the view of philosophy most consistently represented in and by the writings of John Wisdom. It derives from Wittgenstein.
- ²⁰ Of course you can *say* (the words), "When I ask whether an action is voluntary I do not imply that I think something is special about the action". You can say this, but then you may have difficulty showing the relevance of *this* "voluntary" to what people are worrying about when they ask whether a person's action was voluntary or whether our actions are ever voluntary. We might regard the Oxford philosopher's insistence upon ordinary language as an attempt to overcome (what has become) the self-imposed irrelevance of so much philosophy. In this they are continuing – while at the same time their results are undermining – the tradition of British Empiricism: being gifted pupils, they seem to accept and to assassinate with the same gesture.
- ²¹ This latter distinction appears in two senses of the expression "establishing a rule or standard". In one it means finding what is in fact standard in certain instances. In the other it means founding what is to be standard for certain instances. "Settle" and "determine" have senses comparable to those of "establish".
- ²² It is perfectly possible to maintain that *any* "justifications" we offer for our conduct are now so obviously empty and grotesquely inappropriate that nothing we used to call a justification is any longer acceptable, and that the *immediate* questions which face us concern the ultimate ground of justification itself. We have heard about, if we have not seen, the breaking down of convention, the fission of traditional values. But it is not a Continental dread at the realization that our standards have no ultimate

justification which lends to so much British and American moral philosophizing its hysterical quality. (Such philosophy has been able to take the death of God in its stride.) That quality comes, rather, from the assumption that the question of justifying cases is on a par with (appropriate in the same context as) the question of justifying norms.

- ²³ Though in another context we might have. Imagine that before chess was introduced into our culture, another game – call it Quest – had been popular with us. In that game, played on a board with 64 squares, and like chess in other respects, the piece called the Damsel had a fickle way of moving: its first move, and every odd move afterwards, followed the rule for the Queen in chess; its even moves followed the rule for the Knight. It may be supposed that when people began to play Quest, it often happened that a game had to be stopped upon remembering that several moves earlier a Queen was permitted a Knight's move. The rule for the Queen's move might then have been formulated in some such way as: You must move the Queen in straight, unobstructed paths...
- ²⁴ Perhaps this difference provides a way of accounting for our tendency sometimes to think of laws as rules and at other times to think of them as commands. This may (in part) depend upon where we – i.e., where our normal actions – stand (or where we imagine them to stand) with respect to the law or system of laws in question. It may also be significant that when you are describing a system of laws, you are likely to think of yourself as external to the system.
- ²⁵ But this requires a great deal of work. We must have a better description of the "class" and the function of "modal auxiliaries", and we need an understanding of what makes something we do "another" action and what makes it "part" of an action in progress.
- ²⁶ "Must" retains its logical force here. Kant may not have provided an analysis sufficient to sustain his saying that "a deposit of money must be handed back because if the recipient appropriated it, it would no longer be a deposit"; but Bergson too hastily concludes that Kant's explanation of this in terms of "logical contradiction" is "obviously juggling with words". (See Bergson's *The Two Sources of Morality and Religion*, New York, 1935, p. 77.) The difference between your *depositing* and simply *banding* over some money has in part to do with what you mean or intend to be doing – and with what you *can* mean or intend by doing what you do in the way you do it in that particular historical context. We may, following a suggestion of H. P. Grice's (in "Meaning", *Philosophical Review*, July, 1957), think of the actions of depositing and of accepting a deposit as complicated "utterances": you intend that what you do shall be understood. Then it will not seem so extraordinary to say that a later "utterance" (*viz.*, appropriating the entrusted money) contradicts a former one (*viz.*, accepting a deposit).
- ²⁷ Cited in *Hoyle's Rules of Games*, ed. Morehead and Mott-Smith (Signet Book, New York, 1949).
- ²⁸ Some philosophers who employ the notion of a rule have given the impression that there are. What I am trying to argue is that there aren't, but that the analogy is still a good one. One of the claims made for the concept of a rule is that it illuminates the notion of justification; and critics of the concept argue that it fails in this and that therefore the concept is unilluminating in the attempt to understand moral conduct. I think both of these claims are improper, resulting in part from the failure to appre-

ciate differences (1) between rules and principles, and (2) between performing an action and making some movements. The concept of rule does illuminate the concept of *action*, but not that of *justified action*. Where there is a question about what I do and I cite a rule in my favor, what I do is to *explain* my action, make clear *what* I was doing, not to justify it, say that what I did was well or rightly done. Where my action is in accord with the relevant rules, it needs no justification. Nor can it receive any: I cannot *justify* moving the Queen in straight, unobstructed paths. See John Rawls' study of this subject, "Two Concepts of Rules" (*Philosophical Review*, January, 1955). My unhappiness with the way in which the analogy is drawn does not diminish my respect for this paper. For a criticism (based, I think, on a misunderstanding) of the view, see H. J. McCloskey, "An Examination of Restricted Utilitarianism", *The Philosophical Review*, October, 1957.

²⁹ Such an understanding of meaning is provided in Grice (*op. cit.*), but I do not think he would be happy about the use I wish to put it to. A conversation we had was too brief for me to be sure about this, but not too brief for me to have added, as a result of it, one or two qualifications or clarifications of what I had said, e.g., the third point of note 32, note 33, and the independent clause to which the present note is attached.

³⁰ If truth consists in saying of what is that it is, then (*this* sense or source of) necessary truth consists in saying of what is *what* it is. The question, "Are these matters of language or matters of fact?" would betray the obsession I have tried to calm. I do not claim that this explanation of necessity holds for all statements which seem to us necessary and not analytic, but at best for those whose topic is actions and which therefore display a rule-description complementarity.

³¹ Austin's work on Excuses provides a way of coming to master this immensely important idea. The way I have put the point here is due directly to it.

³² Three points about this conclusion need emphasizing: (1) It was reached where the difference concerned isolated *words*; where, that is, the shared *language* was left intact. (2) The tasks to be performed (scraping; chopping; excusing a familiar and not very serious mishap) were such as to allow execution, if more or less crude, with a general or common implement. (3) The question was over the meaning of a word in general, not over its meaning (what it was used to mean) on a particular occasion; there was, I am assuming, no reason to treat the word's use on this occasion as a special one.

Wittgenstein's role in combatting the idea of privacy (whether of the meaning of what is said or of what is done), and in emphasizing the *functions* of language, scarcely needs to be mentioned. It might be worth pointing out that both of these teachings are fundamental to American pragmatism; but then we must keep in mind how different their arguments sound, and admit that in philosophy it is the sound which makes all the difference.

³³ I am not, of course, denying that what you *say* depends upon what you intend to be saying. I am, rather, denying that intending is to be understood as a wanting or wishing. And I am suggesting that you could not mean one thing rather than another (= you could not mean anything) by a given word on a given occasion without relying on a (general) meaning of that word which is independent of your intention on that occasion (unless what you are doing is *giving* the word a special meaning). For an analysis of meaning in terms of intention, see Grice, *op. cit.*

- ³⁴ Or else it is a *special* report, like the one on p. 43 (last sentence); but it is still not a description of my wishes or intentions. Until Austin's *William James Lectures* are published, the best place to find out what a "performative" is remains pp. 142–47 of his "Other Minds", in *Logic and Language*, second series.
- ³⁵ This may be summarized by saying that there is no such thing as *finding out* what a number, etc. is. This would then provide the occasion and the justification for logical construction.
- ³⁶ Cf. D. F. Pears, "Incompatibilities of Colours" (*Logic and Language*, second series, p. 119, n. 2).
- ³⁷ One of the best ways to get past the idea that philosophy's concern with language is a concern with words (with "verbal" matters) is to read Wisdom. Fortunately it is a pleasant way; because since the idea is one that you have to get past again and again, the way past it will have to be taken again and again.
- ³⁸ As Austin explicitly says. See "Excuses", p. 11.
- ³⁹ This sometimes appears to be the only substantive agreement between the philosophers who proceed from ordinary language and those who proceed by constructing artificial languages. But this may well be obscuring their deeper disagreements, which are, I believe, less about language than about whether the time has come to drag free of the philosophical tradition established in response to, and as part of, the "scientific revolution" of the 16th and 17th centuries. I have found instruction about this in conversations with my friend and colleague Thomas S. Kuhn, to whom I am also indebted for having read (and forced the rewriting of) two shorter versions of this paper.

Erratum:

Vol. 1, 1958, No. 3, p. 163 lines 9–11 *read*:

"The difference between the ordinary use and the philosophic use hinges rather upon the alleged fact that the ordinary man applies the word "voluntary" almost exclusively to actions which . . ."

EPISTEMOLOGICAL NOTES ON RECENT STUDIES OF SOCIAL PERCEPTION

by

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"Object attainment" versus "active categorization"

As a point of departure, let us compare two apparently opposed outlooks on the perceptual process: Egon Brunswik's psychology "*vom Gegenstand her*" and George Kelly's psychology of "*personal constructs*". According to Brunswik's basic assumptions, perception of physical as well as social attributes is focussed upon *distal stimulus variables* (9, p. 4) "defined independently of effects or representation on the sensory surface of an organism in a certain situation", variables that (9, p. 3) "can be strictly determined by external measurement". And Brunswik continues: "Environmental conditions even more remote may be called *covert distal* or second-order distal variables . . . , they are exemplified by the I.Q's, "objective" likeabilities, etc., of "social objects" viewed by an organism . . ." Because of this *reliance upon external, objective measurement and the definition of stimulus variables independently of their representation on the sensory surface of the perceiving organism*, Brunswik's model may be characterized as a "*psychology of the empty organism*". Perceptual learning, according to Brunswik, is essentially a learning of a variety of probabilistic cues to distal variables in such a way as to allow for optimal "object attainment", i.e., *veridical estimates of or discriminatory responses to* instances of universal physical and social attributes.

In contrast to Brunswik, Kelly's approach seems to be based upon the assumption that *the foci of social perception are idiosyncratic*. This assumption is reflected in his "individuality corollary" (15, p. 103), which maintains that "persons differ from each other in their construc-

tion of events". Each person lives in his own unique social world, with a perceptual organization of social events different from all other people. Concretely: when a person is asked to *sort* people he knows, he may spontaneously classify two such stimulus persons as *similar*, and *different from* a third stimulus person. If he then is encouraged to try to verbalize *in what respect* the two stimulus persons are similar, he may possibly answer (cf. 15, p. 139) that *they both remind him very much of one particular significant person he knows (Mary)*, whereas the third stimulus person does not. The attribute with respect to which discriminations were made seems in this case to be "Mary-ness", a highly idiosyncratic "trait" representing a *subjective categorization of the appearance and behavior of other persons due to the perceiver's unique previous experience*.

Kelly's individuality corollary and ample demonstrations of persistent spontaneous idiosyncratic discriminatory responses to social objects are thus representative of an outlook on social perception that may be labelled "*a psychology of the empty social world*": the distal foci are conceived of as products of *active categorization of social objects* rather than externally given, universal attributes that can be assessed by measurement, independently of the perceiver's unique "internal" psychological characteristics.

Diagrammatically, the two approaches may be presented as in figure 1.

An essential common feature is *the assumed distal focus of perception*. Neither Brunswik nor Kelly requests the subject to make a minute description (or prediction) of specific overt behaviors of the other person,

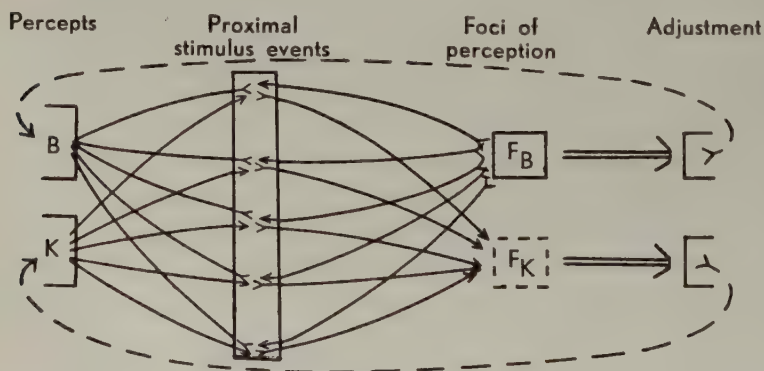


Fig. 1.

Perception by a Brunswikian "empty organism" (B) and by an organism in a Kellyan "empty world" (K).

as sometimes is the case in studies of accuracy of interpersonal perception (18), but are primarily interested in covert distal attributes. Furthermore, both of them are functionalists in the sense that they assume that the perceptual organization is continually being modified by the perceiver's experience of behavioral consequences of his discriminations (arrows leading from "adjustment" to "percepts").

The crucial difference, however, pertains to the definition and empirical anchoring of distal foci: whereas Brunswik's subject is requested to attain the externally defined and measured attribute F_B (arrows *from* the solidly delimited F_B *to* organism), Kelly's subject is allowed to discriminate among social objects with respect to an attribute F_K , the latter representing an active and unique *selection among* and *organization of* proximal stimulus events (arrows *from* organism *to* the vaguely delimited F_K).¹ From Brunswik's point of view, therefore, a core problem of perceptual learning pertains to the mechanisms by which the organism "*decodes*" the probabilistic proximal informations concerning the given, instrumentally relevant, attribute F_B . Kelly's crucial problem pertains to the person's active *coding* of stimulus events so as to *construct attributes* or *arrive at abstractions* that prove instrumental in his personal adjustments.

The controversy illustrated by contrasting Brunswik and Kelly thus seems to lead us right into the dilemma *par excellence* in perception theory: the old realism-nominalism issue, this time partly disguised in terms of methodological preferences, is revealed in differential priorities of problems, and mixed up with the idiographic-versus-nomothetic approach controversy. Whereas Brunswik's "object attainment" is most nicely demonstrated in psychophysical judgement of object size, "active categorization" seems to be the rule when Kelly's patient is freely sorting significant persons he knows.

An ingenious solution to this controversy seems to be provided by a combination of *physicalism* and *transactionalism*. If we conceive of the distal foci of perception as entities, the proximal manifestations of which are *more or less strictly determined by physical "laws"*, we may possibly distribute distal attributes along a continuum ranging from "strict physical determination" to "a minimal amount of physical lawfulness". At one extreme we would then find variables like weight, size, etc., the perception of which may be predicted with a very high level of confidence, once we know the physical characteristics of the stimulus input and the neuro-physiological mechanisms of the sensory surface. Near the other extreme we would find attributes like beauty, intelligence, etc.,

the perception of which cannot be predicted with any confidence at all on the basis of known neuro-physiological mechanisms and of "physicalistic" information about stimuli only. And somewhere in the middle we shall then probably have to locate distal attributes like "anger", "anxiety" etc.

The main characteristic of this continuum is then *decreasing existing physical knowledge of the mediation processes*. For "visual object size", we are able to *specify a variety of cues* (such as size of retinal image, various cues revealing distance, etc.). Furthermore, *functional relationships between the attribute and a given cue are "explained" in terms of universal physical "laws"* (for instance, laws of optics for the relationship between object size, distance, and retinal image). Since for most esthetic and social attributes we are only partly successful in specifying and describing the relevant cues in the language of physics, a similar explanation of the relationship between attribute and mediating proximal stimuli is more or less futile. It is partly achieved, however, for certain emotional states (i.e. overt signs of fear, anger, etc. being explained in terms of glandular activity regulating muscle tonus). Accordingly it is hard to maintain the position that our experience of all social attributes are of a basically different nature than our perception of physical objects. Instead of adopting a dualistic viewpoint by interpreting impressions of "Mary-ness" as products of "Einfühlung" and judgements of *object size* as determined by physical laws, it seems more reasonable to search for a unitary description of all perceptual processes by which both the *coding* and *decoding* aspects are taken care of.

Such a description can be given in terms of the general formula $P = f(O, E)$: the percept (P) is a function of the organism (O) and the environment (E). For our present purposes, however, we shall make a further specification of O and E. By O we want to refer to "*unique internal characteristics of the individual organism*" and by E we shall mean "*environmental influence, described in physicalistic terms*". In this revised transactionalistic formula we may then simply *assign successively more weight to the O factor as we go from stricter to less strict "physical order" in the lens of proximal manifestations of the distal attribute (from "object size" to "Mary-ness")*. In other words: the organism "compensates for" lack of physical order by active, more or less idiosyncratic categorization. A similar interpretation is offered by Bruner in a presentation of the relationship between his "perceptual hypothesis" theory of perception and personality theory. Bruner maintains (4, p. 134): "In sum, the less 'ambiguous' the information, the

less the effect of past experience in confirming hypotheses and the greater the use of input formation." And he further maintains (4, p. 137): "It is perhaps in the perception of attributes of the social environment that people differ most strikingly. For in this sphere hypotheses are strong, and adjustmental consequences serious."

The simple and superficial explanation provided by our revised transactionalistic formula, however, does not seem to fit in very well with experimental evidence. First, *constancy research* in physical perception provides ample demonstration of internal "compensatory" or "stabilizing" mechanisms at work whenever "lawful" proximal mediation of strictly physical attributes is deceptive. My experience of the "whiteness" of this paper at noon and in the evening, in spite of great variations of light intensity, represents a most convincing testimony.

Secondly, individual differences in discriminatory responses to stimulus objects do not seem to increase in any orderly fashion as we approach the "lack of physical order" end of the stimulus continuum. Our studies (17) show that discriminatory responses to complex social stimulus patterns presumably mediating covert distal attributes like "intelligence" and "honesty" are surprisingly uniform in a culturally homogeneous group. Additional demonstrations of high uniformity with respect to spontaneous categorization of complex social stimulus events are found in From's studies of social perception (10). When exposed to film strips depicting apparently rather "chaotic" human behavior, From's subjects spontaneously tend to attribute needs, intentions, and motives to the behavior. From comments on these findings as follows (10, p. 158)²: "In the same way as we may predict the behavior of things under certain conditions by simple observation of them, we may experience a person's behavioral possibilities in such a way that it is more or less spontaneously given what that man can and cannot do.

It is characteristic of these experienced qualities of others that they frequently . . . are given implicitly in my experience of the other . . ."

Also of great interest are studies of apparent behavior by Heider and Simmel (13). The almost universal attribution of "human motives" to their moving geometrical figures and the *amazingly high consensus among subjects with respect to specific human characteristics (like aggressiveness, courage, helplessness etc.) attributed to the different geometrical figures*, show that even covert distal attributes located near the "lack of physicalistic order" end of our hypothetical continuum may be fairly lawfully related to proximal stimuli.

It is important to note that the categorization in studies like those of

From and *Heider and Simmel* occurs spontaneously: it is revealed in the subject's free description of stimulus events, and apparently *phenomenologically* as "real" and "immediately given" as fairly simple "physical" attributes. From (10, pp. 173—4) even speaks of "perceptual constancy" in this field of social perception. And Heider (12, p. 360) seems to conceive of the tendency of attributing motives and stable "traits" to persons as a universal and strong disposition in the human being to respond to any other person as a *causal center* ("Wirkzentrum") in which all his behaviors have their common origin and by which apparently heterogeneous behaviors acquire unity.

An implication of this emphasis upon active categorization in the interpretation of perceptual processes seems to be a *loosening up of traditional boundaries between perception and reasoning*. This is characteristic of Bruner's recent work on "perceptual hypotheses" and "perceptual readiness" (5), and also of F. H. Allport's tentative formulation of "a structural theory of set dynamics and interaction" (1). Bruner, Goodnow and Austin maintain (7, p. 9): "The act of identification involves a "fit" between the properties of a stimulus input and the specification of a category." And Allport states (1, p. 407): "The passage of the set into full perception involves (1) the addition of certain items from the stimulus-object or from effector contacts with the environment, items that complete the *structure*, and (2) such increase of energies in the aggregate, or structure, as may be afforded by the items just mentioned."

Percepts,³ according to Allport and Bruner and associates, are therefore largely dependent upon *internal psychological characteristics other than neurophysiologically known sensory mediation mechanisms*. They are very explicit on this point, referring to the formation of perceptual categories as (7, p. 8) "an act of rendering similar by coding operation rather than a forcing of equivalence on the organism by the nature of stimulation . . . In so far as each individual's milieu and each culture has its own vicissitudes and problems, might one not expect that this would reflect itself in the way in which members of a culture will group the events of their *physical and social environments*?" (Italics ours.)

By raising the problem of *active categorization of the physical environment* as well as the social environment, Bruner and associates seem to suggest a solution to the nominalism-realism issue characteristic of the "transactional" approach by the Ames group. In a programmatic presentation of the latter, Ittelson and Cantril make the following statements:

"In perceiving, parts of our own experience are attributed to events external to ourselves in whose independent existence we firmly believe." (14, p. 4).

"The world as we perceive it is the product of perception, not the cause of it." (P. 5).

"... the purposes we attribute to other people we are dealing with are just as "real" as the characteristics we attribute to objects." (P. 8).

"... it is impossible to have any perception which is devoid of symbolic content." (P. 19).

"... perceiving refers to the process by which a particular person, from his particular behavioral center, attributes significances to his immediate environmental situation. And the significances which he attributes are those which he has discovered have furthered his purposes." (P. 26).

In spite of their explicit *programmatic* rejection of physicalism, however, Ittelson and Cantril maintain (14, p. 11): "It is important to note, however, that we are concerned... not with externality as it may exist in its own right, in the sense of the mystical essence of things, nor with externality as perceived, but rather with externality in so far as it is *knowable to the physical sciences* and as it enters into the transaction with relevance to the living organism." (Italics ours.) Among most ardent "transactionalists" like Ittelson and Cantril, therefore, there seems also to be an ultimate recourse to physical concepts "in whose independent existence they firmly believe", even though they refer to prominent physicists like Einstein and Infeld, who maintain that (14, p. 11) "physical concepts are free creations of the human mind and are not, however, it may seem, uniquely determined by the external world."

When reviewing our own findings, Heider and Simmel's studies of apparent behavior, and From's studies of "spontaneous experience" of qualities like motives, etc., in other persons, we are left with a definite impression that (a) certain covert social attributes may be phenomenologically as "real" as comparable complex physical attributes and (b) the inference from mediating stimulus events to covert attribute under certain specified conditions seems to be equally predictable for physical and social perception. In view of these findings, therefore, we feel tempted — instead of extending existing "laws" for physical perception to the field of perception of other persons — to make a bold attempt at a search for a common denominator for all instances of perceptual learning.

If we believe in the physicist's statement that physical attributes are "free creations" rather than entities possessing a higher epistemological status than social attributes, *we are ultimately forced to explain the formation of perceptual categories like "object size", etc., in exactly the same way as "hostility" and "intelligence"*. In other words: as soon as we leave pure sensations within one specified modality and go on to study complex perceptual organization, Brunswik's *restricted* functionalism does not seem to be adequate for our purposes. The restriction pertains to his request that the distal attributes must be assessed by external measurement, independently of the organism's discriminatory responses. According to this request, Brunswik's "empty organism" lives in a world in which all distal attributes are *a priori* given. Successful adjustment, furthermore, depends upon veridical attainment of these attributes. Throughout a long series of more or less successful instrumental discriminations, therefore, the organism improves in his decoding of social and physical events.⁴ The modification of perception due to behavioral consequences of discriminatory behaviors is thus heavily restricted by the nature of the "true", externally assessed "Gegenstände".

An organism living in a world characterized by a less restricted functionalism is initially exposed to stimulus events that may *potentially* (in terms of biologically possible alternative neurophysiological organizations) reveal a number of different attributes. His first and primary lesson pertains to an organization of events (a formation of perceptual foci) that is instrumentally relevant. In other words: his categorization of a recurrent type of aggregate of stimulus events is *passively restricted by the nature of his sensory surface, central nervous system, etc., and actively dictated by the differential behavioral consequences of (biologically possible) alternative perceptual organizations*. A potential attribute therefore acquires (phenomenologically) "reality" if perceptual focussing upon (or discrimination of events with respect to) that attribute "pays off" in terms of behavioral consequences.

In general, one specific attribute X is "betrayed" or revealed in a variety of stimulus events $e_1 \dots e_n$ (cf. fig. 2). Most of these events, however, are *composite* stimulus events of which only one component (x) is a potential cue for X, some of the remaining components representing potential cues (y, z, ...) for other attributes (Y, Z, ...). The instrumental relevance of X, then, is clearly a function of *the extent to which neglect or misperception of the x-components in $e_1 \dots e_n$ has behavioral consequences invoking negative sanctions*.

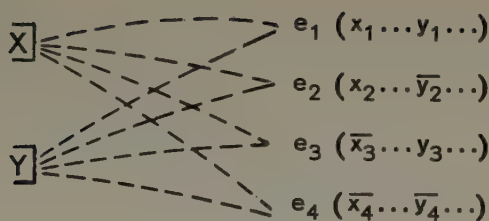


Fig. 2.

Behavioral events (e_1 - e_4) providing simultaneous "clues" to two attributes (X and Y)

Concretely: two children, A and B, are brought up in environments that are strikingly different with respect to value orientations. A, being the son of a famous football star, lives in a social environment in which physical strength and athletic abilities are of major concern. Let us assume, for the sake of simplicity, that his father has invested much personal prestige in his own athletic career. Furthermore, his admiration for outstanding athletic performance is reflected in his social life in the sense that the people he associates with (mostly other athletic stars) are honored (flattered, responded to in a submissive way, etc.) in proportion to their athletic abilities. A's behaviors are sized up and responded to in the same way, by and large: every potential symptom of high athletic ability arouses praise, varying in subtlety from overt, verbally expressed evaluations to subtle facial expressions of delight and pride.

B, on the other hand, is the son of a man who happened to invest most of his personal prestige in an academic career. Because of personal frustrations in the field, however, this investment has now been transferred to the son, the emphasis upon intellectual values being reflected in his social life and his responses to B's behaviors in roughly the same way as the significance of athletic ability seems to pervade interpersonal relations in A's environment.

Let us now examine in some detail the behavioral settings in which A and B are exposed to instances of the social attributes "athletic ability" (X) and "intelligence" (Y). Let us, again for the sake of simplicity, assume that A and B themselves do not differ markedly with respect to the two attributes, i.e., they are approximately equally clever at athletics and equally bright, and *initially* equally strongly motivated toward intellectual and athletic achievement. In situations in which both physical ability and brightness are required, they are accordingly exposed to approximately equivalent social sanctions. For instance, when either of

them makes a wonderfully planned and brilliantly performed tackle in a soccer game, he is rewarded in terms of direct or subtle praise from his father (e_1 in fig. 1). By the same token, when either of them fails to solve a mechanical task requiring both manual dexterity and ingenuity, he is "punished" in terms of signs of disappointment, etc. on the part of his father (e_4 in fig. 1).

Differential sanctions in response to the same behavior, however, will be the rule when the behavior is reflecting a high level of X and a low level of Y and *vice versa* (e_2 and e_3 in fig. 1). Concretely: in a choice situation in which A continues reading a book on popular science in spite of the fact that his playmates are eagerly persuading him to join them in going skiing, his father may make subtle derogatory remarks to the effect that his son seems to be a "sissy" or a "book-worm". Irrespective of the detailed nature of the evaluative responses, they are likely to be negative, whereas B in response to the same behavioral events will experience praise.

In another situation of the same kind, e.g. either of the boys making slightly insulting remarks to a visitor who happens to be an intellectually somewhat inferior sports star, differential sanctions will again be evoked. In A's home, such behavior is probably regarded as a "crime", whereas equally insulting behavior toward a physically weak and handicapped intellectual genius may be ignored. In B's home, the opposite holds true. And if we now examine separately for each environment the evaluative responses to respectively x- and y-components, we find:

(a) In A's environment, x is always positively evaluated and \bar{x} is without exception negatively evaluated.

(b) Furthermore, in A's environment, y is half of the time positively evaluated and half of the time negatively evaluated. The same holds true for \bar{y} . In general: the evaluative response to events reflecting Y (intelligence) *will each time be contingent upon specific additional components or features of the event.*

As a consequence, *the prerequisites for the formation of an "operational concept" of Y seem to be absent in A's environment.* Events reflecting Y jointly with a series of different additional attributes (Z...) must be responded to as *unique events*, because predictable social sanctions are not tied to y-components of events at all. On the other hand, events reflecting X may be fairly adequately responded to by identifying (or "abstracting") the x-component each time, ignoring detailed stimulus characteristics and unique aspects. In general, the more essential a biologically possible categorization is to a person's adjustment in his

social environment, the more likely he is to develop a perceptual focus corresponding to that categorization.

From this point of view, it does not seem too difficult to understand the *spontaneous* and *highly uniform* categorization of apparent and real social behaviors in studies by Heider and Simmel and by From. Whereas the instrumental relevance of certain subtle and specific personal characteristics may vary greatly from environment to environment, more basic and general categorizations are certainly of universal instrumental relevance. For instance: the child's attribution of *emotional states* to other significant persons seems to be almost as essential to improved adjustment to other human beings as the attribution of distance to physical objects is to successful motor locomotion. Mother's angry voice and/or abrupt movements and/or stern face, etc. represent, irrespective of all additional aspects of the stimulus situation, a fairly valid warning that the intended excessive movement and shouting will not be tolerated. The "smallness" of the distant attractive toy and/or the multitudes of things interspersed between the child and the toy similarly represent a warning that many steps will be required in order to reach it. In both cases, the development of a perceptual focus represents a detection of certain *invariant behavioral consequences* of specified constellations of discrete stimulus events.

Such *invariant behavioral consequences* thus seem to be the essential condition for the development of a perceptual focus on a potential attribute. A highly relevant experimental demonstration is provided by the transfer experiments by Lawrence (16), in which he found evidence of the development of a perceptual focus ("acquired distinctiveness") *only when a specific geographically separated attribute of the rat's physical environment had been systematically covaried with goal achievement*. Stimuli mediating an equally "real" attribute⁵ that had been randomly related to goal achievement was *not* differently organized after repeated presentations, as far as the transfer data showed. Formally nearly identical experiments by Hammer on human discrimination learning, designed to test the validity of the above mentioned surprising findings in the Lawrence study, yield equally conclusive results. Hammer concludes (11, p. 50): "There was no evidence that responses of any sort were acquired to the irrelevant elements during the discrimination training."

It seems to us that such fairly unequivocal findings have not been paid proper attention by students of perception in recent discussions of the nominalism-realism issue. Both Lawrence and Hammer seem to be

puzzled by them: the findings do not seem to fit their implicit model of perceptual learning of the Brunswikian "restricted functionalism" kind at all. Indeed, it looks as if a heavy prejudice in favor of a Newtonian realism prevents us from drawing the proper epistemological implications from experimental evidence contrary to our "realistic" attitude. The physicist speaks of his concepts as "free creations" — biologically *possible* and highly useful modes of coding our environment — but as students of perception we tend to believe more in the "objective reality" of the physical world he describes and measures than in the epistemological interpretation he offers.

If for a moment we adopt a broad historical perspective on the development of the natural sciences, however, we are faced with a most fascinating and puzzling problem. Since (a) our physical world as described by the modern top scientist represents after all only a continually revised and refined version of primitive man's categorization of physical events, and since (b) the latter obviously did *not* represent the proper description of "reality", by what criteria can we find out *when* in the history of science *active, instrumentally relevant coding of physical events* was replaced by a *decoding of an "external reality"*?

From an ontogenetic point of view, furthermore, we are struck with the *extremely high instrumental relevance* of certain physical attributes. Quite apart from their epistemological status in terms of *external and objective measuring devices*, attributes like time, distance and space seem to be a *sine qua non* for basic human adjustment. Their instrumental relevance is universal:⁶ unless the child learns to code his environment in terms of spatial-temporal relations he will be utterly helpless, without the basic prerequisites for more complex readjustive responses to physical and social events. Such "basic" physical attributes are *interrelated phenomenologically* — the "largeness" of a building being experienced by the child who is walking around it in terms of "a long distance to go" and "a long time since I started" —, *frequently mediated through several modalities* — "the toy feels large" (haptic size) and it "looks large" (visual size) — and *prerequisites for the proper coding of a variety of significant social attributes* (the child's understanding of physical locomotion being necessary for the attribution of certain *motives* to another person).⁷ In short: they are instrumentally relevant in a vast majority of the situations encountered in our waking life.⁸

The "*experienced reality*" of basic physical attributes thus seems to be closely related to their universal over-all (indirect and direct) instrumental relevance. The same seems to apply to more basic social attributes

like *motives* and *emotional states*: until the child acquires a mode of perceptual organization of the behaviors of other persons by which he codes apparently heterogeneous behaviors into categories like "mother wants to . . .", "she wants me to . . .", etc., he will be most helpless in his adjustment to the surrounding "social reality". Physical attributes of less universal instrumental relevance, for instance esthetic attributes such as "harmony", "style", etc., as *defined and uniformly judged by a homogeneous group of competent artists*, are accordingly experienced as "less real" by the average man (brought up in an esthetically fairly "poor" environment). To the ardent esthete, however, whose major motivation is the creation and enjoyment of the arts, those very attributes may constitute the "real essence of human life". The perceptual lens (cf. fig. 1) is accordingly most appropriately conceived of as an *intra-organismic coding mechanism* by which *potential* external attributes (biologically possible modes of perceptual organization of stimuli) become psychologically "real". Perceptual categories are therefore *relative* (2), restricted by *the structure of receptor organs and central nervous system and learned perceptual organization*.

By including restrictions due to *learning*, we want to stress the distinction between "*potential attributes*" and "*attributes focussed upon*". The description "*attribute X does not exist for organism O*" applies, according to our point of view, equally well to the deaf person's inability to experience pitch, to a color-blind person's inability to experience certain chromatic attributes, and to a hypothetical "untouched" African's inability to experience beauty and intelligence *as defined by leading Hollywood "star hunters" and leading American psychometricians respectively*. Perception of an attribute requires the presence of attribute-relevant stimulus input *and* the operation of a specific coding mechanism. The absence of the latter — whether due to *lack of learning* or obvious *structural deficiencies* — explains the organism's inability to make the appropriate discriminations.

Learned coding mechanisms, furthermore, even though they have so far hardly been investigated at all by neuro-physiological methods, obviously have their neurophysiological correlates. High consensus among culturally homogeneous subjects in perception of covert distal attributes like "goodlookingness", "intelligence", "honesty", etc. therefore presupposes uniform neurophysiological modifications due to *common types of previous experience*. Even though we know very little about neuro-physiological aspects of the perceptual integration of stimulus items in such cases, we have no reason to believe that it is less "lawful" than

seemingly more "simple" physical perception. In brief, we cannot find any convincing evidence in support of any ingenious solution to the nominalism-realism issue according to which Brunswik's empty organism is conceived of as a *de-coder* of a "physicalistic reality" and Kelly's patient is considered an *active coder* of "chaotic" social events.

Our distinction between "*potential attributes*" and "*attributes focussed upon*" and our assumption that only *instrumentally relevant potential attributes become foci of perception* seem to imply an epistemological outlook on the perceptual process different from Brunswik's "probabilistic functionalism". We conceive of perceptual learning as an acquisition of *instrumentally relevant coding mechanisms*, restricted and partly determined by biological factors, but at the same time actively determined by differential *behavioral consequences of alternative, biologically possible, modes of perceptual organization*. By doing so, however, we do not choose the nominalistic horn of the old *philosophical* dilemma. We do not deny "the reality" of object size and human anger, but rather assume the existence of equally "real" potential attributes that are not perceived at all. And the essential difference between *the attributes I experience as "real"* and *those potential "real" attributes I do not experience at all* is to be explained largely in terms of differential behavioral consequences of the two respective classes of biologically possible coding mechanisms.

Some problems pertaining to the assessment of veridicality of perception

Our deviation from Brunswik's "restricted functionalism" is revealed in an emphasis upon the *relativity of foci perception*. This relativity is perhaps most clearly demonstrated in the as yet largely unexplored field of social perception, strikingly in Kelly's studies of personal constructs (15) and partly in our own experiments on the sorting of multi-dimensional social stimulus objects (17). Since we do not believe that relativity is unique for social perception, however, this general relativistic outlook has certain implications for the assessment of veridicality of perception of physical objects as well.

We have elsewhere (17) discussed briefly the "experimenter-centered fallacy" in the assessment of veridicality of social perception. When the perceiver's estimate of an attribute deviates from the "objective" measurement of it, "misperception" may frequently be due to perceptual focussing on *aspects other than those defined by the experi-*

menter. Such problems, however, seem to be equally important in studies of perception of physical objects with a symbolic meaning, for instance in "New Looks" studies of perception of size of coins (6) and experiments such as the one by Bruner and Minturn (8) on perceptual identification of a briefly exposed ambiguous visual stimulus pattern (representing a compromise between the number 13 and the letter B).

In the latter case, "correctness" of perceptual identification can only be determined if the experimenter is willing to make an arbitrary decision concerning the *intended meaning* of the stimulus. In a real life situation where the same ambiguous pattern occurs in an important coded military message, however, the individual's perceptual identification of it will be immediately judged as "correct" or "incorrect" *depending upon the "real" intended meaning of the sign*. "Correct" identification, furthermore, may be a matter of life and death. In such cases, therefore, assessment of veridicality of perception presupposes investigations of "intended meaning" of proximal stimuli and behavioral consequences of alternative identifications.

The great interest in the early "New Looks" experiments pertained in large part to the interpretations by which such phenomena as overestimation of size of coins were seen as determined by the individual's emotional and motivational states. Whereas Brunswik's studies of *size constancy* clearly suggest that the organism seems to restore centrally what has been distorted at the proximal level, Bruner and Goodman's findings were initially interpreted as the demonstration of an opposite mechanism: a central distortion of simple psychophysical judgement. Brunswik comments on this as follows (9, p. 14): "The range of ecological variables contributing to judgement of magnitude . . . goes so far as to include such socially conditioned and emotionally or motivationally loaded factors as the monetary value of coins . . ."

However, we may be a little hesitant in interpreting the subject's adjustment of the size of the circle as an unequivocal measure of his identification of the *magnitude* of the coin. His habitual perceptual focus on coins in everyday life obviously refers to *monetary value*. Accordingly, the apparent "perceptual distortion" may possibly be as adequately described as *halo effect due to "bifocal" perception*, in the same way as the Müller-Lyer illusion may be described (9, p. 14) as a "*length-area assimilation*". According to this description, the discrepancy between instrumental perceptual response and externally measured attribute does not testify to an irrational distortion, but to *the persistence of a firmly established and highly instrumentally relevant perceptual focus*, in spite

of a socially induced set aiming at perceptual focussing on another attribute.

In all experiments requesting *unidimensional judgements of multidimensional objects*, therefore, a systematic examination of *potential attributes other than the one to be judged* seems to be a prerequisite for a more thorough understanding of eventual “nonveridical” judgements. In most cases we have sufficient knowledge of the physical attribute under investigation to draw *negative conclusions*, i.e., we know that normal adults have established perceptual foci on *physical magnitude*, etc. Our knowledge of potential irrelevant attributes is most meagre, however. As a consequence, we are justified in concluding that a particular subject in the given specific experimental setting (like Bruner and Goodman’s experiment) fails to “attain” the attribute *we demand him to focus upon* as adequately as would be the case in other settings when certain irrelevant attributes are absent, but we are at a loss when trying to explain the apparent distortion in terms of specific psychological mechanisms.

Another major issue in this context, of particular importance in assessment of veridicality of “unidimensional” social judgement, is highlighted by *the sometimes striking lack of correspondence between verbal description of an attribute and actual discriminatory behaviors* in our studies of social perception (17), and by the very puzzling finding that some subjects in the experiments *spontaneously sorted the stimulus persons with respect to their level of intelligence when ranking them as potential friends, but failed to do so when explicitly instructed to rank them with respect to level of intelligence*. — Is it possible that veridical perception of an attribute X may “*occur without awareness*” in a vaguely instrumental setting, even though the subject is unable to make a veridical *conscious inference* from the same mediating stimuli to the covert attribute?

If so, the testing of veridicality of perception in clearly judgmental situations does not provide sufficiently unequivocal information concerning the individual’s general discriminatory capacity. In interpreting the findings, we are at least obliged to consider *the possibility of a distorting effect of the superimposed goal of accurate unidimensional judgement*: when there is a verbal “mislabeling of” and/or a particularly strong emotional response conditioned to an attribute, the “making conscious” of that attribute by verbal description prior to the perceptual process may possibly interfere with a firmly established unconscious coding mechanism so as to decrease accuracy.

The assumed relativity of attributes and the emphasis upon behavioral consequences of discrimination thus seem to make for a systematic reformulation of problems of veridicality. Once we leave the judgmental situation, where we are under certain conditions allowed to *define* veridicality in terms of correspondence between perceptual response and "external assessment" of the attribute under investigation, we are faced with a number of crucial problems. Since we are no longer allowed to measure *efficiency of de-coding*, veridicality becomes, as Bruner puts it, primarily a matter of "model-building", or rather, a matter of examining the "objective" instrumental relevance of alternative coding mechanisms.

This presupposes a systematic analysis of means-ends relationship and individual motivation. Concretely: a school principal wants his students to get good grades, and he is screening potential new students with this goal exclusively in mind. A star hunter from Hollywood is urgently searching for a new actress for a movie that he hopes will save him from financial ruin. A High School student is seeking out friends in a new environment with the implicit and unconscious motivation of receiving affection and reassurance of his feeling of social approval. A child is wondering whether he shall first approach his mother or his father in order to get the new bicycle he wants so intensely. In all these cases the "*adjustmental veridicality*" of the sorting of other persons ultimately depends upon the differential instrumentality of various alternative coding mechanisms relative to a specified goal.

Thus, the high school principal makes an "adjustmentally" veridical sorting of new students if he maximizes the probability of good grades. Ignorance of their esthetic attributes and "sex appeal" may be highly instrumental in that case, whereas it may be fatal to the movie producer. By the same token, the intellectual characteristics of other persons may be largely irrelevant to the high school student's ultimate goal of a personal feeling of social acceptance. In brief, *the different discriminatory behaviors cannot be compared at all with respect to veridicality*. And, more important: the evidence of the *selectivity* of the perceptual process in most such instrumental settings strongly suggests the necessity of a systematic mapping of extremely complicated relationships between potential alternative attributes and specified behavioral goals. The relativistic outlook on foci of perception therefore ultimately implies a joint — and possibly *unitary* — analysis of "cognitive" and "conative" aspects of psychological phenomena, suggested by Brentano's definition of them (3, p. 116) as "...solche Phänomene, welche *intentional* einen Gegenstand in sich enthalten." (Italics ours.)

NOTES

- ¹ F_K can therefore only be objectively assessed and described *post facto*, by a systematic investigation of those social objects that have been sorted by the subject. Until such an investigation has been made, the experimenter has to rely exclusively upon the subject's introspective account of the attributes.
- ² Translated from Danish.
- ³ A percept is *defined* by Allport as (1, p. 23): "A phenomenological experience of an object, ... the way some object or situation appears to the subject as dependent upon his own organism, as observer-involved, non-denotative, and "private" ..."
- ⁴ Cf. the increasing perceptual size constancy with age (9, p. 19).
- ⁵ "Reality" being defined in terms of (a) external measurement of physical stimulus properties, and (b) biological adequacy of sensory and neural structures required for the "coding mechanism".
- ⁶ Cultural differences in time perception discussed by von Bertalanffy (2), even though highly relevant and interesting, do not at all question the universal instrumental relevance of a general categorization of "time".
- ⁷ Of particular interest in this context are the almost universal "anthropomorphic" descriptions of certain moving geometrical figures in the experiment by Heider and Simmel (13).
- ⁸ It is interesting to note that the proper spatial-temporal categorization frequently seems to fail in dreams, when motor adjustment is not essential at all.

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MATES ON REFERENTIAL OPACITY

by

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INTRODUCTION

In his well-known paper "Synonymy"¹ Professor Mates not only discusses the problem of synonymy and its relationship to interpretation, but he also comments upon the views of Professors Quine, Naess, Lewis, and Carnap.

His criticism of the two last-mentioned philosophers stresses the importance of belief-sentences for the question of synonymy, and has initiated a vivid discussion.² His criticism of the two first-mentioned philosophers has, however, not been commented upon as far as I know. In the case of Naess this is understandable, as Mates merely points out a fairly obvious discrepancy between his proposed criterion of synonymy and two of the tentative definitions of that term mentioned by Naess in his *Interpretation and Preciseness*.³ In Quine's case, however, Mates' criticism has far more serious implications.⁴ His argument against Quine shows, if it is correct, that Quine's treatment of the problem of substitutions in contexts governed by 'believes that' has the very undesirable consequence of making this problem dependent upon the nominalism-realism dispute.

The aim of this paper is to show that Mates' argument is invalid, and moreover, that any other argument to the same end is bound to fail, as the conclusion is false.

To show this, I shall first review Mates' argument, then find its defect and show why Quine's treatment of the problem does not turn on the nominalism-realism dispute.

1. Mates' argument

Mates starts by sketching Quine's view:⁵

Consider the sentences:

- (1) Philip believes that Tegucigalpa is in Nicaragua.
- (2) Tegucigalpa is the capital of Honduras.

Substitution into (1) on the basis of the true identity sentence (2) gives:

(3) Philip believes that the capital of Honduras is in Nicaragua.

To explain the non-equivalence of (1) and (3) Quine says that the occurrence of 'Tegucigalpa' in (1) is not "purely designative"⁶ and claims that only purely designative occurrences of names are subject to substitutivity on the basis of a true identity sentence.

To show that this explanation is not a good one, Mates considers the following parallel case:

(4) Philip believes that $2^{10} < 1000$.

(5) $2^{10} = 1024$.

(6) Philip believes that $1024 < 1000$.

Granted that (4) and (6) need not be equivalent, we should, according to Quine, say that some occurrences of ' 2^{10} ', like the one in (4), are not purely designative, whereas others are. And Mates continues:

"This, unfortunately, commits us to a kind of Platonism which no one is more anxious than Quine to avoid. He apparently wishes to assert that expressions like ' 2^{10} ' *never* occur designatively, for he is of the opinion that there are no such things as numbers. Now if they never occur designatively, substitution on the basis of a true identity sentence will never be possible. Hence, this sort of explanation is difficult to combine with nominalism. I should make it clear, however, that I do not object to Quine's view because it clashes with nominalism; my objection is rather that this particular problem may be treated independently of the nominalism-realism dispute."⁷

2. The failure of Mates' argument

Mates is right in maintaining that the parallel case to which he draws attention cannot be treated by Quine without commitments to a kind of Platonism.

But he is wrong in believing that these ontological commitments are due to Quine's mode of explanation. They are made already before the explanation sets in, at the very beginning, when (5) is said to be a true identity sentence.

To see this, we merely have to observe how Quine's explanation works: We are presented with the three sentences (4), (5), and (6). (5) is said to be true, (4) and (6) to be non-equivalent. That (5) is true, means that ' 2^{10} ' and '1024' designate the same (abstract) object. In all purely mathematical contexts the one expression may therefore be substi-

tuted for the other, *salva veritate*. This may be done in many other contexts too; and wherever it can be done, the expressions are said to occur purely designatively. In all other contexts, i.e. contexts where the substitution cannot be performed without affecting the truth, the occurrences of the expressions are, according to Quine, not purely designative. We may not substitute '1024' for the occurrence of '2¹⁰' in (4) *salva veritate*. From this we conclude that this occurrence of '2¹⁰' is not purely designative.

But by doing this we do not introduce new ontological commitments. Mates is quite right in observing that this mode of explanation leads us to say that some occurrences of '2¹⁰' are not purely designative, whereas others are, and that it hence presupposes a platonic ontology. What Mates is not aware of is that such a designative occurrence of '2¹⁰' is to be found already in the sentence (5). (This follows immediately from the transitivity of identity). So it is the assertion of (5), and not Quine's mode of explanation, that commits one to Platonism.

Mates is, by the way, also quite right in observing that if expressions like '2¹⁰' never occur designatively, substitution on the basis of a true identity sentence (like (5)) will never be possible. But he fails to note that this impossibility is a very thorough one indeed. There would simply be no true identity sentence like (5) to serve as a basis for the substitution.

The fallacy of Mates' argument becomes especially clear if we suppose that he poses his problem to somebody who is of the opinion that there are no such things as numbers. Such a person would not see any need for explanation. He would not regard (5) as a true sentence, and would therefore, unlike Mates, not consider the non-equivalence of (4) and (6) a problem.

To make Mates' example work we therefore have to find a way of reading (5) which is acceptable to such a person. This person will not regard (5) as true if it is read "2¹⁰ and 1024 are the same object" or "'2¹⁰' and '1024' designate the same object", since he is of the opinion that there are no such things as numbers. To make him acknowledge (5) as true, we have to find a way of reading (5) which does not commit him to an ontology containing numbers. Nobody has been able to free mathematics of this commitment to abstract objects. But if somebody should some day find a way of doing this, then this would save Mates' example. It would, however, not help Mates' argument. Our nominalist could perfectly well continue to talk about "designative" occurrences of '2¹⁰' without forsaking his nominalism. He could even

continue to read (5) as “‘2¹⁰’ and ‘1024’ designate the same object”. He only had to explain away his usage of the term ‘designate’ and derived terms as a mere manner of speaking. And our alternate reading of (5) would provide him with a way of doing this.

We hence see that Quine’s mode of explanation enables us to treat Mates’ problem independently of the nominalism-realism dispute. The reason is, as we repeatedly have observed, simply that the occurrence of ‘2¹⁰’ in the sentence (5) is designative.

Mates’ argument is therefore fallacious. Its failure lies in Mates’ lack of concern about the ontological commitments which he makes when at the beginning of his argument he presupposes that sentence (5) is true.

To see that any argument to the same end is bound to fail, we now merely have to notice that whenever Quine’s mode of explanation leads us to explain failure of substitution by saying that a certain occurrence of an expression is not purely designative, the expression, already before the explanation set in, occurred purely designatively in some context, viz. on the one side of an identity sign in an identity sentence which was said to be true. Hence the ontological commitments were made already there, and were not introduced by the explanation.

In spite of its distinction between “purely designative” and “not purely designative” occurrences of expressions, Quine’s mode of explanation therefore does not introduce new ontological commitments.

Appendix: Mates’ objections against Quine’s method of ascertaining ontological commitments

Mates ends his article with some comments upon the relevance of synonymy to the problems of interpretation and philosophical debate.

In need of an example of philosophical argument based on questionable interpretation, he again turns to Quine, this time to his method of ascertaining ontological commitments.

Mates sketches Quine’s view and finds it quite clear that it rests upon the assumption that sentences beginning with the existential quantifier ‘($\exists x$)’ are to be interpreted by sentences beginning with the phrase ‘there is an entity x such that’. As far as Mates can see, this assumption is without justification. Mates gives two reasons for this. First the existential quantifier may be read in many different ways, and not all of these ways are intended by their users as always introducing ontological assertions. Secondly it is possible to interpret existential quantifiers

in such a way that no ontological commitments are involved, save possibly commitments to the existence of expressions in the language. In order to indicate how this may be done, Mates points out that the sentence " $(\exists x) (x \text{ is prime and } 5 < x < 11)$ " may be interpreted by the sentence "there is a constant such that the sentence which results from substituting this constant for 'x' in the matrix 'x is prime and $5 < x < 11$ ' is true."⁸

Mates' first objection against Quine, or at least objections essentially the same, are discussed by Quine in *From a logical point of view*⁹ and therefore shall not concern us here. Not so with second objection, to which Mates devotes much more space. In spite of its prominence, this objection proves, as we shall see, nothing against Quine, but reveals instead an important misconception by Mates.

Let us consider Mates' example. The sentence

(a) $(\exists x) (x \text{ is prime and } 5 < x < 11)$

may, as Mates correctly observes, be interpreted by the sentence "there is a constant such that the sentence which results from substituting this constant for 'x' in the matrix ('x is prime and $5 < x < 11$ ') is true". It all depends — unfortunately for Mates' argument — on how we interpret the open sentence

(b) $(x \text{ is prime and } 5 < x < 11)$

If we interpret (b) as "which is such that the sentence which results from substituting it for 'x' in the matrix 'x is prime and $5 < x < 11$ ' is true", then we come to interpret the closed sentence (a) the way Mates does it in his argument. (The open sentence (b) is with this interpretation satisfied by an expression in our language, namely '7'. And as Mates correctly remarks (utilizing some unspecified method of ascertaining ontological commitments — he might well have used Quine's), the assertion of (a) with this interpretation does not commit us to the view that there are numbers).

If we interpret (b) as "which is prime and between 5 and 11", then we come to interpret the closed sentence (a) the way it is done, e.g. in classical mathematics¹⁰. (The open sentence (b) is with this interpretation satisfied by nothing but the number 7, a universal, if such there be, and consequently the assertion of (a) under this interpretation commits us to the view that there are numbers.)

The difference between the two interpretations of (a) is therefore in its entirety due to the difference between the two interpretations of the open sentence (b) and has nothing to do with the existential quanti-

fier. Neither has the difference anything to do with Quine's method of ascertaining ontological commitments, which, as we have seen, works well in both cases and hence is untouched by Mates' argument.

In order to save his argument, Mates would have to show that according to Quine's criterion of ontological commitments, the assertion of (a) commits us to acknowledge the existence of objects which do not satisfy (b). And to show this is not easier than it is to solve the problem of squaring the circle.

The reason for this failure of Mates' argument is, to epitomize, simply that Mates has not quite realized the following point: Whereas the quantifier tells us *that* ontological commitments are involved¹¹, the open sentence following it tells us *which* these commitments are.¹²

NOTES

¹ Benson Mates: "Synonymity". In *Meaning and Interpretation*, University of California Publications in Philosophy, 25 (1950) pp. 201-226. Reprinted in Leonard Linsky, ed.: *Semantics and the Philosophy of Language* (Urbana, 1952), pp. 111-136.

² See, e.g. the long series of papers by Carnap, Church, Pap, Putnam, and Sellars, reviewed by Rulon Wells in the *Journal of Symbolic Logic*, 20 (1955), pp. 293-296.

³ Mates points out that according to two of Naess' tentative definitions all logically equivalent sentences are synonymous. I do not doubt that further empirical investigations will show that people very seldom, if ever, say e.g. that all analytic sentences are synonymous. (Doubters are advised to practise their principles in a discussion involving such sentences, e.g. a mathematical discussion). I therefore regard Mates' criticism of Naess as good. As to Mates' own criterion, from the point of view of empirical semantics it is apparently too strong. A discussion of this and related problems is found in Naess: "Synonymity as revealed by intuition", *Philos. Review*, 66, 1957, pp. 87-93.

⁴ In his paper, Mates does incidentally also criticize Quine's method of ascertaining ontological commitments (pp. 222-3, Linsky pp. 132-3). This criticism will be examined in an appendix at the end of this paper.

⁵ Mates, p. 213 (Linsky, p. 123).

⁶ In his later writings, Quine prefers to call such occurrences 'purely *referential*' (cp. *From a logical point of view*, esp. pp. 140 ff.). In order to change Mates' argument, and even his wording, as little as possible, I have in this paper (except in its title) kept to the old term '*designative*'.

⁷ Mates, p. 214 (Linsky, p. 124).

⁸ Mates, p. 223 (Linsky, p. 133).

⁹ PP. 105-7.

¹⁰ That this interpretation and not that of Mates is the natural one in classical mathematics is due to a theorem of Cantor's to the effect that there must be classes, in particular classes of linguistic forms, having no linguistic forms corresponding to them. (Georg Cantor: "Ueber eine elementare Frage der Mannigfältigkeitslehre." *Jabres-*

bericht der Deutschen Mathematiker-Vereinigung, 1 (1890–91), pp. 75–78. Reprinted in *Georg Cantor Gesammelte Abhandlungen* (Berlin: Springer, 1932). Cf. Quine: "On Universals", *Journal of Symbolic Logic*, XII (1947), p. 78, or *From a logical point of view*, p. 121).

Quine's use of (a) – in a passage quoted by Mates – to illustrate how classical mathematics affirms that there are universals, is therefore not placed in a bad light by Mates' alternative interpretation of (a). (And Mates probably does not intend to put it in such a light.) Incidentally, even if Quine's interpretation of (a) had happened to be unjustified, this would not have saved Mates' argument about the interpretation of quantifiers. The argument is, as we shall see, bound to fail.

¹¹ For more precision, see Quine: *From a logical point of view*, pp. 13, 103–7, 117–8, and 128–9; and, e.g. Quine's article "Designation and Existence". *Journal of Philosophy*, XXXVI (1939), pp. 708–9.

¹² I am indebted to Professor Dallas Laskey and Dr. Jaakko Hintikka for a number of suggestions on language and style. I am especially grateful to Professor Benson Mates, whose kind comments have led me to insert the words 'by Quine' in the first paragraph of section 2, and to rewrite the appendix in order to take proper care of the word 'primitive' in footnote 18 of Mates' paper.

COMMENTS ON THE FOREGOING

by

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By way of an answer I shall attempt to restate with greater clarity the points to which Mr. Føllesdal takes exception, and in the process I shall offer a few second-thoughts on the subject of oblique contexts generally.

In one of its many forms Leibniz's Law appears as follows:

- (L) For any sentences S , S' and terms t , t' , if S' is the result of replacing one or more occurrences of t in S by occurrences of t' , then S' follows from S and $\ulcorner t = t' \urcorner$.

This intuitive principle seems to break down in certain cases, which Frege, Russell, Carnap, Quine and others have tried in various ways to explain. Quine, in particular, has attempted to clear up a number of the apparent counter-examples by observing that in each such instance the term t does not occur purely designatively in the sentence S , whereas the principle, rightly understood, applies only to occurrences of t which are purely designative. Thus, that (3) does not follow from (1) and (2) is to be explained by saying that the term "Tegucigalpa" does not occur purely designatively in (1). Now we may observe that (6) does not follow from (4) and (5) either, and one would expect a parallel explanation; but here we run afoul of Quine's view that numerical expressions *never* occur purely designatively, since abstract entities like numbers do not exist.¹ Thus it is difficult along this route to give a plausible account of the fact that "2¹⁰" and "1024" are interchangeable in some contexts and not in others, and indeed that interchangeability breaks down for numerical expressions in precisely the same kinds of contexts (belief-contexts and oblique contexts generally) in which it breaks down for expressions like "Tegucigalpa". Of course one can give

an explanation that is not parallel, or one can decide to use the words "purely designatively" in such a way that expressions which do not designate anything may nevertheless occur purely designatively.² In any case, if ontology is relevant to whether or not a given expression occurs purely designatively, then the concept of "purely designating" does not appear to be very useful for clarifying the conditions under which expressions may be interchanged in oblique contexts. It would seem better to use concepts like "coextensive" and "synonymous", the applicability of which is much wider and need not raise questions of ontology, and to explain the fact that only synonymous expressions can be interchanged in oblique contexts by pointing out that in such contexts we usually claim to present, not the very sentence which would be used by the speaker (believer, doubter, etc.) nor merely a sentence that has the same truth-value as his, but rather a sentence synonymous with the one he would use.

Apropos of Mr. Føllesdal's comments it may be noted that (L) applies to all sentences S , S' , $\lceil t = t' \rceil$, whatever their truth-values may be. In giving apparent counter-examples to (L) it is natural *but not necessary* to choose S and $\lceil t = t' \rceil$ true and S' false. If in place of (4), (5), (6) I had used

(4') Philip believes that 12 is greater than 9.

(5') $12 =$ the number of the planets.

(6') Philip believes that the number of the planets is greater than 9.

I should perhaps not have led Mr. Føllesdal into concluding that the failure of my argument "lies in Mates' lack of concern about the ontological commitments which he makes when at the beginning of his argument he presupposes that sentence (5) is true." My assumption of the truth of (5) was inessential, though I am hardly inclined to apologize for it. All that the argument required was an apparent counter-example to (L) which was of the same form as (1)–(3) and in which t was a numerical expression.

I should like to add that many of the discussions of oblique contexts, and this applies especially to "Synonymity", fail to give attention to the numerous cases in which words occur directly though in oblique contexts.³ In the sentence

That gullible juror actually believes that
the murderer had nothing to do with the crime.

the words "the murderer" seem to occur directly. The truth of the sentence does not depend upon whether the juror would assent to "the murderer had nothing to do with the crime" or to any synonym thereof; the sentence means only that the man who is in fact the murderer is believed by our gullible juror to have had nothing to do with the crime. Various pragmatic factors determine more or less definitely whether, in a given situation, an expression occurring in an oblique context is to be taken directly or obliquely.⁴ Whether the expression designates or not, is clearly not the crux. In the case of belief it is more a question of *whose words*, the reporter's or (something like) the believer's, are being used to characterize what is believed; other cases can be treated analogously. It is interesting to note that this ambiguity invades not only "psychological" contexts but other contexts as well. For example, observe the parallelism among the following:

⌈It is believed that A is B ⌋ sometimes means ⌈What is in fact A , is believed to be B ⌋.

⌈ A is necessarily B ⌋ sometimes means ⌈What is in fact A , is necessarily B ⌋.

⌈If A were B ⌋ sometimes means ⌈If only what is in fact A , were B ⌋.

⌈ A was B ⌋ sometimes means ⌈What is now A , was previously B ⌋.

It seems to me that any satisfactory explication of oblique contexts will have to take account of such cases as these.

In the Appendix to his paper Mr. Føllesdal directs attention to pp. 222—3 of "Synonymity". When I there mentioned the possibility of interpreting the sentence " $(\text{Ex}) (x \text{ is prime and } 5 < x < 11)$ " by "there is a constant", etc., I was (should have been?) arguing generally against the criterion as stated on p. 222, and specifically against the notion that if one uses undefined quantification over numerical variables, then one makes an ontological commitment to numbers. Mr. Føllesdal's observation that under the given interpretation the sentence still asserts the existence of *something*, if not that of numbers, is thus not to the point. I must admit, however, that the obscurity of the text gives ample occasion for such misunderstandings.

NOTES

- ¹ In thus interpreting Quine, I was of course relying on articles published before 1948 and had no foreknowledge that some of them would reappear, with relevant changes, in *From A Logical Point Of View*.
- ² In "Notes on Existence and Necessity" Quine tells us that "Pegasus" never occurs designatively, presumably because it does not designate anything.
- ³ Among those to whom this does not apply are Russell ("On Denoting", *Mind* XIV (1905), p. 489), Smullyan ("Modality and Description", *Journal of Symbolic Logic* XIII (1948), pp. 31-37), various medieval logicians, and possibly Aristotle.
- ⁴ But even when they determine in a given situation that the expression is to be taken directly, they need not determine that a coextensive expression will be taken directly if it is substituted for the given expression.

REASON IN SOCIETY AND MODERN LOGIC

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Is really modern mathematical logic an adequate framework for the explication and formalization of the kind of reasoning which occurs in everyday life in society, as well as with regard to the kind of more refined reasoning that is represented by the social scientist (the historian, the sociologist, the social psychologist, the literary historian, etc.)?

To our knowledge this problem has not been raised explicitly in its full scope. Certainly the English analytic philosophers seem to assume the inadequacy of modern formal logic when it is applied to everyday reasoning. But this is rather an implicit assumption, which as such certainly is very important in Wittgenstein's *Philosophical Investigations*. But it is more the mistrust of formalization as such than a mistrust of any specific type of formalizations.

There has been some tendency to *apply* the modern calculi to social matters, but so far it is rather difficult to point to any fruitful results. Sometimes, of course, one finds formulations like this: "Given the values V_1 V_2 V_3 and the two action-series Aa and Ab , then the following is true: the action Aa_1 by the person P_1 will probably realize the value V_1 to degree 3, the value V_2 to the degree 5 and the value V_3 to the degree 3."¹ But this is not the application of a calculus, it is an idiomatic peculiarity, that *might* have a heuristic value.

The development of modern logistics has brought two old disciplines into disrepute, at least in some quarters. These are *classical logic* and *dialectics*. It has often been said that classical logic has turned out to have lost much of its interest. There is, though, a steadily growing minority which denies this, not always for the same reasons. They argue that those working within logistics have claimed too much for their discipline. According to *von Freytag-Löringhoff*, there are several logistic

calculi, but only one logic. What he means seems to be that logic is concerned with that language in which one talks about any calculus, and that language is not in itself a calculus, it is the ordinary language. And:

Doch kaum ist der Kalkül eingeführt, so wendet ihn der Logistiker gegen die Sprache, misst sie an seinem Vorbild, wirft sie Unklarheit, Unexaktheit, mangelnde Ausdrucksfähigkeit für logische Nuancen, Unkontrollierbarkeit und anderes mehr vor. Alle diese Vorwürfe kommen im Grunde darauf hinaus, dass die lebendige Sprache eben nicht Kalkül, nicht auf Mathematik zugeschnitten, denkend und verstehend, nicht mit Zeichen blind rechnend ist, Ausdrucksmittel ist und nicht Beweismittel.²

This is certainly not very different from the kind of criticism that many English analytic philosophers would make.

Mathematical logic in itself is philosophically neutral. When it sometimes appears not to be, then it is because it has been used as a weapon in philosophy, especially by the logical positivists. This, like most formal ingenuity, has not failed to have an influence. But it has also influenced logistics itself. Thus it is hardly believable that so many logicians would defend the thesis of extensionality, if it were not for metaphysical reasons. When it was introduced by Wittgenstein³ it was for reasons that belong to metaphysics — thereby he proposed to eliminate entities like subjects and souls — and not for reasons belonging to pure logic. As Russell says, the thesis of extensionality is sought maintained for several reasons, among them the reason that only then can the theses of logical behaviorism and physicalism be maintained.

Now, ordinary language is simply not truth-functional. One has tried to reformulate in truth-functional terms expressions that in ordinary language are not truth-functional, but so far one has hardly succeeded, and there is a growing tendency now to think that one never will succeed in this enterprise.

I shall not go into this problem in detail here, since I have already discussed it elsewhere.⁴ Rather I shall proceed on the assumption that one has not been able to eliminate expressions which are not truth-functional from the language, nor to reformulate them in truth-functional terms.

Popularly, the fact that a language is not truth-functional means that in certain contexts the intensions of terms are more important than their truth-value. This is the case in contexts of the form 'P believes that p is the case', "'p'" means p, 'it is possible that p'. In all these cases what matters most is the *meaning* of p, not its truth-value.

We shall now turn to that kind of reasoning which most often occurs in society, and which finds its more refined forms in social science.

Let us then take up some of the more elementary notions in symbolic logic. Extensions and intensions of terms are complementary. Thus a certain property corresponds to a class, namely the class of individuals that have this property. If f is the property that corresponds to the class A , then generally this holds:

$$(1) \quad (x)[(x \in A) \equiv f(x)]$$

But as soon as we turn to social matters the "predicates" have a much more complicated structure than those usually used to illustrate the predicate calculus. Let us as an example take the class of believing Catholics. Tentatively we may set down:

$$(2) \quad (x) [(x \in A) \equiv (x \text{ believes that } p, q, r, \dots \text{ are true})]$$

where A is the class of believing Catholics and p, q, r, \dots the dogmas of the Catholic Church.

Here it would be too simple to say that the statement on the right side is of the form

$$f(x)$$

This would only be an escape from the problem. Neither would it help to say that the right side statement is of the form

$$xRy$$

because a dyadic relation delimits a class of ordered pairs, and the class of believing Catholics is not a class of ordered pairs, but a class of individuals.

Further, by writing out the statement on the right side it becomes still more complex. One Catholic dogma is that of papal infallibility. Thus we get by taking that in:

$$(3) \quad (x) [(x \in A) \equiv (x \text{ believes that } p, q, r, \dots \text{ and that when the Pope speaks } \textit{ex cathedra}, \text{ what he then says is absolutely true.})]$$

Now, I think that what those who hold that the modern calculi are adequate also for social science have to show is how to formalize contexts like those above. Hitherto it has not been done, and as far as it has not been done or perhaps cannot be done, so far the student of

man and society is not too much helped by modern logic when he attempts to give account of his own way of thinking.

Let us look somewhat closer at the structure of those contexts that we used as examples above. If we have an object-language, as it is called, where the predicates describe simple properties like 'red' 'green', the right side of our formulas will have the form

(4) $f(x)$

But in social science the right side will rather have the form

(5) P believes that $f(x)$.

Or, more complicated (there seems to be no limit to the complications of this kind):

(6) P_1 believes that $f(x)$ is true because P_2 said that $f(x)$ is true.

Or:

(7) P_1 believes that P_2 had heard from P_3 that $f(x)$ is probably true.

Thus if (4) is a statement in an object-language, then (5) is a statement in a "meta-language", (7) a statement in a meta-meta-language (and also containing a modal statement) etc.

From this we shall draw one important conclusion: if one is going to study the language of social science, it is far from sufficient to study the structure of any object-language. *One has rather to study the hierarchy of languages itself.* Or, the language of social sciences is not any specific object-language, but the hierarchy of languages.

Now, if one really tried to do this, I suspect that one would arrive at something analogous to Hegel's logic, or perhaps more analogous to the structure of his *Phenomenology of Spirit*. Perhaps this may explain why Hegel has had a more profound influence upon the studies of man than it seems likely that the modern development in logistics will ever have.

NOTES

¹ Inquiry, Vol. I, No. 1, p. 52.

² *Logik. Ihr System und ihr Verhältnis zur Logistik* (Stuttgart/Köln 1955), p. 190.

³ *Tractatus Logico-Philosophicus*, p. 143.

⁴ Cf. my *Objectivism and the Study of Man* (forthcoming). Important discussions of this problem are to be found in B. Russell: *An Inquiry into Meaning and Truth* (London 1951), ch. xix; R. Carnap: *Meaning and Necessity* (Chicago 1947), pp. 141-2; A. Pap: *Analytische Erkenntnistheorie* (Wien 1955), pp. 13 ff., pp. 23-26; M. Macdonald: *Philosophy and Analysis* (Oxford 1954), articles by Carnap and Church.

SOCIOLOGY OF FAITH

Werner Stark: *The Sociology of Knowledge*. The International Library of Sociology and Social Reconstruction, Routledge and Kegan Paul, London, 1958. 36s. net. 356 pp.

Dr. Stark has written a useful book about an interesting subject. This book, which incidentally is too long and would have profited from some pruning, is at its most interesting when it discusses and describes the views of various people who undertook substantive or analytic studies in this field. I shall, however, not concentrate on this, the picaresque aspect of the book, its wanderings amongst the highways and byways of gnosis-sociological thought, but shall extract and discuss the central thread of continuous argument which, camouflaged by prolixity and many digressions, is to be found in this work.

One of the ways in which the author believes himself to be advancing the subject is by drawing a distinction between sociology of knowledge proper and the theory of ideology: the failure to make this distinction has, he thinks, bedevilled the past treatments of the subject. But what differentiates them? It appears (p. 46) that the doctrine of ideology "... is ... different in its nature because it is a *psychological* rather than a sociological discipline" (italics mine). It is true that the study of *individual* prejudice, the psycho-pathology of *self-deception*, are not a part of sociology, but who uses the term ideology to designate it? On the contrary, although the term may be used pejoratively in the sense of *false* consciousness, it definitely implies that the illusion in question is collective, institutionally supported, and quite compatible with individual, psychological normality, rationality and freedom from self-deception. That is both the customary and the useful employment of the term "ideology". I find it hard to see any merit in Stark's re-

definition, other than the dubious one that it helps him attain certain favoured conclusions.

The picture which he presents is: on a more specific level there are 'ideologies', factional, false and motivated by concrete interests and behind them 'perspectives, vantage-points of whole societies', all of them true and mutually complementary, and transcendental, (for rigorously testable subjects are exempt from this schema). Only the latter, the vantage-points, are the proper objects of sociology of knowledge. Dr. Stark attempts to support or establish the distinction by an example, but the example seems to me to show merely that we all hold or presuppose views of differing levels of abstraction, that opponents may share the same presuppositions at one level and not at another, and that where our views have social roots, different ones may be relevant at different levels.

What Dr. Stark is trying to do also emerges from his definitions. He maintains (p. 49) "If all men could and would come to control their subconscious and rise superior to the insinuations of selfish or sectional interests, the doctrine of ideology would die off, because there would be no more raw material left in the contemporary world for it to study." But, (p. 80) "the whole truth — will include *no* ideology and *all* socially determined knowledge (Dr. Stark's italics): ... socially determined knowledge is part and parcel of it (truth), indeed, constitutive of it — only ... different aspect-structures ... must be fused ... to give the full truth ..." But this attempt to define ideology in terms of both falsehood *and* of factional or selfish motivation, and the object of sociology of knowledge proper in terms of truth *and* of the whole or "supra-functional" society, must break down. Not all factionally motivated convictions are false (on the contrary, the closeness of conviction to some practical interest gives them a better chance of verification.) Not all outlooks of whole societies can be true, unless we have a truly amazing faith in some remarkable synthesis or mishmash of all apparent contradictories. The background outlooks, just because they are about transcendent or categorial matters that are not easily checked, are in fact particularly liable to be swayed by interest. How large or supra-functional must a group be before it is above 'factional' interest? What is a society, what is a faction, in, for instance, our contemporary world? In a society with a ruling class, how does one distinguish the society's outlook and the biased outlook of its ruling class, especially when the others do not attain articulateness? If they are not to be identified, are we then, when talking of the social outlook, speaking merely about something

latent, an ideal but unrealised synthesis, or are we talking about some common presuppositions, something wholly inexplicit? But may not these too have motives, or be false, or inconsistent? Thus not only does Dr. Stark smuggle his conclusions into his definitions, but also these definitions turn out to be unworkable ones.

Another questionable distinction introduced by the author is between what he calls macro- and micro-sociology of knowledge. His official definition of their difference (pp. 20 and 21) makes micro-sociology the study of "the narrower world of scholarship and art", whilst macro-sociology "fixes its attention on the inclusive society and its influence." He considers both to be useful, but the latter to be more fundamental. When however we get a fuller picture of the two kinds, in the course of refutations of writers who preferred micro-sociology (J. Kraft and F. Znaniecki, p. 28), it emerges that Dr. Stark thinks of micro-sociology as so to speak an internal study of who believes what and why in a given society, whilst macro-sociology concerns itself with societies *as units*. But either this distinction is entirely relative, a matter of degree or numbers, or, if a fundamental difference is allowed, Dr. Stark will come up against two things: the charge of reifying abstractions, and the problem of how non-arbitrarily to delimit his units for analysis and comparison (see below). The possibility of the former, nominalistic criticism he dismisses in extremely cavalier fashion as "... materialism so crude as to rule itself out of court". By under-rating what he calls micro-sociology of knowledge Dr. Stark in effect turns his back on what makes sociology of knowledge important and interesting, namely the institutional and organisational aspect of ideas: something that both 'ideologies' and 'vantage-points, perspectives' of whole societies, have in common. This brings out that Dr. Stark's correlation of ideology with falsehood and motivation, and of perspectives with truth and freedom from faction, is untenable.

So having dismissed the study of ideology as a psychological concern, and partly dismissed the institutional aspect of ideas as the subsidiary subject of "micro-sociology", we are left in the dizzy heights of outlooks of whole or "supra-functional" societies. Why — *cui bono*? It is difficult to resist the temptation to indulge in some speculative ideological analysis oneself, and seek the motive. Notoriously, the kind of sociology of knowledge conceived by Dr. Stark often leads to relativism and despair of the attaining of true knowledge. But Dr. Stark is not led to this. On the contrary: we are led into a strange night where all cows are, for a change, white or off-white. Dr. Stark in effect believes that

although knowledge is socially conditioned, *all* societies grasp the truth, though only partial aspects of it. On page 155 we are sternly told that "We are far from anxious to 'indict, secularise, ironize, satirise, alienate, devalue': these pastimes are for the market-place, not for the study, for the propagandist, not for the scholar." I should have thought that the combatting of error, delusion and superstition was the work of both study *and* market-place, and easier in the former.

His doctrine that "every society, and every other supra-functional subsociety as well, can lay claim to truth, the substance of truth, though only one aspect of it" (p. 155) is later given the name *metectic*. But allowing this even for *sub*-societies. Dr. Stark's view comes up sharply against the problem he would in any case have had to face in view of the difficulty of delimiting societies: is then the doctrinal conflict between subsocieties (and societies) a mistake, an illusion, or even a necessary illusion *à la* Hegel? Are disagreements between groups only apparent, waiting for sociology of knowledge to illuminate an underlying harmony, or is even the conflict a part of a deeper unity? For, contrary to what the "metectic" view seems to imply, holders of rival views generally consider those views incompatible both logically and in their concrete implications, and not without reason; and even whole societies sometimes change their outlooks in the belief that they are exchanging a falser for a truer one. The problem is also high-lighted by the fact that Dr. Stark's universal tolerance is not *quite* universal, some cows retaining their more habitual nocturnal greyness: "Positivists of the nineteenth century, having cramped their style of thought, were in no position to tackle (the issue of theism versus atheism) successfully, or even sensibly". (footnote, p. 159). All (sub)societies have a partial grasp of truth, but some less so than others, it appears.

Like many of those whom the diversity of outlooks led to a sceptical relativism, Dr. Stark exempts from his own — quite different — conclusion the more rigorous kinds of knowledge: "... the absolute truth of the multiplication table need not be brought into doubt — nor that of the more recondite propositions of the mathematical kind". (p. 161.) No partial truths here — only one truth, and outside it error. Where superficially this does not appear to fit there is a way out, "... their mathematics was like ours, but was overlaid with magic: the physics underneath their metaphysics was in no way different from any rational physics." Possibly so: but the sharp separation, of the rigorous kinds of truth, universal and unique, on the one hand, and on the other, the metaphysico-religious kind, where the "metectic" view holds and *all*

ideas have a measure of truth, really makes the general argument too easy: "... when primitives make statements which seem to deny a basic law of formal logic, they are not acting otherwise than the most modern logician who is at the same time a good Christian. He, too, asserts that in the Godhead Three and One are identical, but that does not mean that he denies the absolute truth of the law of contradiction. He has merely decided not to carry it from physics into metaphysics, and this is quite legitimate ..." (p. 162). Quite a lot appears to be legitimate *in* this field, but not the denial of the field altogether: "To us it seems clear that positivism ... cannot provide a true representation of the facts in their entirety because it presses the methods appropriate only to the study of one sector of reality (nature) beyond their proper sphere, and we should always unambiguously condemn it ..." (p. 181).

Dr. Stark notices the inconsistency, the arbitrary asymmetry, of condemning just this view, and exempting it from "metectic" tolerance. On the next page (182) he says "This ... is an opinion as legitimate as any other ... But the curious fact is that men have never been able to live with it." He then proceeds to prove the practical untenability of total relativism, of a complete acceptance of flux in all things and truths, implicitly confusing this with positivism. But positivism is not identical with it, unless it be proven that no way out of total flux other than a transcendental one exists, *and* that the transcendental escape route is a possible one — and he proves neither. On the contrary, the grandiloquent title of the last chapter, "*The Conquest of Social Determination*" (*italics mine*) turns out to be an empty boast — *such* conquests would make Alexanders of us all. The conquest appears to consist of a very mild recommendation of the metectic theory, the view that different societies perceive differing, incomplete aspects of one unique but transcendent and unknowable higher reality. The rival "pragmatist" doctrine is condemned because "useless as an epistemology ... does not so much as conceive of the possibility of absolute truth where man and his society are concerned" (p. 339) The corresponding defect of the metectic theory is frankly admitted: "... it contains an irreducible element of faith" (p. 342).

Wondering what Dr. Stark would make of one of the earliest doctrines of the extra-individual determination of important knowledge, namely the doctrine of Grace, (which, be it noted, did not "conquer" but accepted such determination,) I looked up St. Augustine in the index. The reference, page 270, did not lead me to an answer to that question, but shed light on the concrete implication of the metectic view:

"Is Christianity not in fact a set of ideas come down from heaven? The present writer would be the last to deny it." (continued in footnote on the same page:) "... though the divine message is a revelation ... of a higher reality, it was ... presented in a form which fitted ... its day and place. We need only think of the parables of our Lord ... To get at that meaning, we must penetrate the outer crust of socially determined ... ideas to reach ... the generically human and ... the divine. The social environment in which it worked ... appears highly significant for the understanding of the divine message itself".

Clerics of various denominations in the United States are nowadays often graduates of the social sciences. Dr. Stark has, for the future, provided them (barring, as he explicitly does, Fundamentalists) with a reason for being sociologists of *knowledge*. Sociology of knowledge is for him, if not identical with theological exegesis, at least its necessary prerequisite and its support.

Thus the essence of Dr. Stark's general argument is this: he excludes the possibility of error in social outlooks by a definition consigning false consciousness to the study of ideology and throws out such a study from the sociology of knowledge proper. He reinforces this by a mistake through which he misses the whole point of sociology of knowledge, namely the mistake of the attributing of falsehood to individual self-deception. He further strengthens the claim that various outlooks are true by excluding the positive and corrigible sciences from them, and by stressing the transcendence, the inaccessibility of this residual truth, and wards off the possibility of the rejection of this kind of truth *in toto* by an appeal, for a change, to a practical consideration. Whether he holds all transcendental views *equally* true, equally metectic, or whether the Christianity to which he subscribes is *more* so, or whether he thinks that other outlooks are a lipping or crypto-Christianity, is not fully clear. Dr. Stark's idea that there might be a world without ideology reminds one of the Marxian hope of a society without alienation: but in the Starkian version, man free from fraction, egoism and delusions might also find himself in a society crowned with a *mélange* of all possible religions and metaphysics, for they are all claimed to be partial views of the truth. Some have preached universal harmony through eclecticism and watering down: others, for instance in anthropology, have claimed to find the universal transcendent truth held in strange clothing and social contexts. Dr. Stark has provided both these activities with a gnosiso-sociological rationale, and he has reintroduced as "sociology of knowledge" the shallow optimism, familiar from time to time in philo-

sophy, which hope to have *and* eat all possible cakes. It is a metaphysics of faith rather than a sociology of knowledge. But this rationale of Higher Eclecticism is logically ill-founded, it travesties the religion which it seeks to defend; and it is unhelpful in sociology.

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CARNAP INTRODUCES SYMBOLIC LOGIC

Rudolph Carnap: *Introduction to Symbolic Logic and its Applications*, Dover Publication, New York 1958, \$1.85, 241 pp.

Prof. Carnap's *Abriss der Logistik mit besonderer Berücksichtigung der Relationstheorie und ihrer Anwendungen* was published in Vienna 1929 as the second (or, from the extensional point of view, the first) volume in the series *Schriften zur wissenschaftlichen Weltanschauung*. Actually, the book had a double function: to facilitate and extend the practice of formalization and axiomatization, and to strengthen the antimetaphysical position by showing that clear thinking can be achieved wherever conceptual structures are worked out explicitly and with care. It undermined confidence in what Carnap labelled "*die an Blutarmut dahin-siechende alte Logik*", which he declared had been dealt its "*Todesstoss*" by logical paradoxes.

The book aroused great enthusiasm in Vienna and other centers, being eminently concise, broad in its application — and cheerful and optimistic in its visions. Since its appearance many things have happened — both in philosophy and logic — which Carnap has instigated, but which also have caused the central frontiers of philosophical research to move beyond his *Abriss*, even in its new editions.

The present book is a slightly modified translation of *Einführung in die Symbolische Logik mit besonderer Berücksichtigung ihrer Anwendungen*, which appeared in 1954 (as a complete revision of the *Abriss der Logistik*). It is meant to be an introductory volume, and as such gives an ample survey, especially for the more philosophically interested rather than the mathematically inclined reader. A brief review of the contents will indicate the scope of the book.

It is divided into two parts, the first giving some systems of symbolic logic, the second presenting some applications, chiefly axiomatic systems

constructed within the frame-work supplied by the first part. The introductory part, which, however, occupies the larger space, gives three closely related symbolic calculi, first a simple language A, which is described rather informally. It contains many illuminating side-remarks on translations from the word language, intension and extension and other topics usually requiring extensive comments on the part of an instructor in an introductory course on symbolic logic. The author early introduces and effectively applies his L-terminology in proof construction and subsequent development. On the whole, proofs are merely indicated; it seems as if Carnap in places wants to exploit the intuitive understanding of and familiarity with the subject which the reader eventually gains. The treatment seems to be very careful; the involved topic of substitution in the predicate calculus may be mentioned in this connection. An extensive list of L-true formulas of the sentential and predicate calculi is given. The present reviewer could find only one of the more commonly included L-true formulas wanting, namely $(\exists x)(Fx \supset Gx) \equiv (x)Fx \supset (\exists x)Gx$, which may indicate that the book on this point is complete for every "practical" purpose. The introductory sections of part one close with a treatment of definitions, predicates of higher levels and functors.

Language B gives in barest outline a formal development to encompass both language A and the further constructed language C, the latter two being more informally described. This chapter opens with some general remarks on the subject of semiotic where the difference between syntactical and semantical considerations is briefly indicated, then proceeds with the construction of a language B as a syntactical system (it is a simple system of types with the λ -operator and the axiom of choice, following Ramsey's simplifications of the *Principia Mathematica*). The notions of proof and derivation are introduced, and some general theorems are given (although not a standard theorem of introductory texts that every tautology (of the sentential calculus) is provable, this theorem is briefly mentioned). Then Carnap constructs B as a semantical system, supplying rules of value-assignment and evaluation. The treatment of language B closes with some further remarks on the relationship between syntactical and semantical systems (here the classical theorems of Gödel are mentioned in a few lines).

Language C is an extension of A; the treatment is quite informal and supposedly easy to follow for a beginner. Language B is the "official" system of Carnap; provability of a sentence in C is understood as provability of its translation into B within that framework. The content of

the chapter on C is very rich. There is a careful and extensive treatment of the theory of relations, the λ -operator, equivalence classes, individual descriptions and various assumptions of finiteness and infinity, just to mention some of the topics expounded.

Part two of the book is given to the application of symbolic logic. First there is a discussion of forms and methods of language construction, treating such subjects as thing languages, coordinate languages and the axiomatic method. Concerning the latter Carnap introduces a distinction between an *interpretation* of an AS (axiomatic system) and a *model* of an AS, the distinction being of relevance in the application of the axiomatic method in the study of empirical sciences, where an interpretation of an AS has to include at least one descriptive constant (if the application is to be fruitful), hence the necessity of introducing semantical considerations and intentions of primitive signs, whereas a model (and a logical interpretation) of an AS needs to utilize only the extension of primitive signs and composite language forms.

Various AS are then constructed within the framework of A and C; it suffices to mention from mathematics AS of set theory, Peano arithmetic, the real number system and several geometrical theories. From physics systems of spacetime topology are given, and from biology some examples are included, chiefly following Woodger in his *Axiomatic method in biology*.

From this (incomplete) recapitulation of the contents of Carnap's book we may conclude that it furnishes a wealth of information, perhaps too rich a picture for the beginner. The book is of very moderate length (240 pp.), so the treatment is necessarily brief, and of some topics rather condensed, but on the whole it is carefully and clearly written. The concepts introduced are informally discussed to help the reader to grasp their significance and to avoid likely misunderstandings. The bibliography is quite extensive and there is appended a short but helpful guide to the literature. The much-used book of Curry, *A Theory of Formal Deducibility*, is not included, otherwise the selection seems satisfactory. The printing appears to be very careful, and misprints are almost non-existent (on page 60 there is an easily detectable error in formula T 14—2 h (2)). There are many exercises, both for routine and of more advanced nature (the latter collected in an appendix to the part on applications).

A review ought not to be only laudatory, therefore we advance some critical remarks.

(1) Carnap seems to be guided by the idea of total applications, of

construction of a comprehensive framework. For many purposes the resulting apparatus is extremely heavy and not easily manipulated (especially not by a beginner). The present reviewer feels that the applications indicated in Carnap's book have a somewhat barren appearance. It is, of course, of great interest to introduce a definition, and a well-adapted definition may be essential to the further development of a theory. But when a definition is introduced, one may want to see how it is used. It may be replied that this would be to transcend the limits of an introductory text. But a comparison may be made with the book of Rosenbloom (also appearing in the Dover Series); it is to be regretted that Carnap has not found space to include something like the beautiful and non-trivial chapter four of Rosenbloom. Carnap introduces a good many definitions, but the results obtained (in contradistinction to what can be obtained) are somewhat meager. Even in an introductory text the student should sense the important results of his field of study and get an appreciation of the power of the methods introduced.

(2) There could perhaps be more on the subjects of consistency, completeness and decidability of the constructed calculi (and subsystems thereof). As regards the border line between effectivity and non-effectivity in constructed methods there are not many remarks. As an example, in rule R 11—1 there is no hint as to which rules are effective and which are not. Also, if one intended to construct methods as simple as possible with regard to the application of symbolic logic, a point of view that apparently does not receive any great attention in the present book, an investigation of decidable subsystems of the constructed calculi might be of interest.

(3) Proofs are seldom elaborated in detail in the book; there are mostly indications and hints of how to proceed. A systematic treatment of inductive definitions and the corresponding proof-methods is not included, although on the meta-level both inductive definitions and proofs by induction are extensively used. A lack of systematic treatment here (and informal side-remarks) also helps to blur the interdependence of symbolic logic and mathematics. Carnap introduces Peano arithmetic as an AS, but he uses the methods of that AS in his meta-level reasonings. There is no paradox here, but a discussion of meta-theory versus object-theory and the need of intuitively accepted modes of inference might have been of great help to a beginner (one recalls here the excellent treatment of Kleene (*Introduction to Metamathematics*) and the very systematic exposition of Curry and Feys in their recent book, *Combinatory Logic*).

(4) There are many comments on semantics in this book, and on the differences between syntactical and semantical systems. But only formal semantics is mentioned. And when applications of symbolic logic to an empirical science is attempted, there is a gap in the exposition of Carnap. There ought to have been some remarks on the necessity of combining empirical semantics with the methods of Carnap's book in actual attempts at applications, especially in claiming relevance of the formal problem, obtained by many and perhaps questionable transformations, to the problem initially considered (compare the present reviewer's discussion, in his *Notes on the application of formal methods in the soft sciences*, forthcoming in this journal).

As regards the possibility of application, mention beyond the brief comments (pp. 41, 42) on how to avoid modalities would have been welcome, and references to the literature (e.g. to von Wright's essay on modal logic) could have been incorporated to advantage.

However, these few critical comments shall not in any way detract from the fact that this book by Carnap is one of the more important introductory texts on symbolic logic available today. Especially for the non-mathematician the work seems to offer an excellent introduction to the study of symbolic logic. It should also be stressed that it is not more than an introductory book. The field of formal logic and its applications is now so vast that there is need of many introductory works to be read by the beginner who desires to make an adequate survey of the subject.

Jens Erik Fenstad.

[illegible]

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